PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

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VOL. LXXIII. For the Year 1783.

PART II.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

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PHILOSOPHICAL TRANSACTIONS.

XVII. *On the proper Motion of the Sun and Solar Syſtem; with an Account of ſeveral Changes that have happened among the fixed Stars ſince the Time of Mr.* Flamſtead. *By* William Herſchel, *Eſq. F. R. S.*

Read March 6, 1783.

THE new lights that modern obſervations have thrown upon ſeveral intereſting parts of aſtronomy begin to lead us now to a ſubject that cannot but claim the ſerious attention of every one who wiſhes to cultivate this noble ſcience. That ſeveral of the fixed ſtars have a proper motion is now already ſo well confirmed, that it will admit of no further doubt. From the time this was firſt ſuſpected by Dr. HALLEY we have had continued obſervations that ſhew Arcturus, Sirius,

Aldebaran, Procyon, Caſtor, Rigel, Altair, and many more, to be actually in motion; and conſidering the ſhortneſs of the time we have had obſervations accurate enough for the purpoſe, we may rather wonder that we have already been able to find the motions of ſo many, than that we have not diſcovered the like alterations in all the reſt. Beſides, we are well prepared to find numbers of them apparently at reſt, as, on account of their immenſe diſtance, a change of place cannot be expected to become viſible to us till after many ages of careful attention and cloſe obſervation, though every one of them ſhould have a motion of the ſame importance with Arcturus. This conſideration alone would lead us ſtrongly to ſuſpect, that there is not, in ſtrictneſs of ſpeaking, one *fixed* ſtar in the heavens; but many other reaſons, which I ſhall preſently adduce, will render this ſo obvious, that there can hardly remain a doubt of the general motion of all the ſtarry ſyſtems, and conſequently of the ſolar one among the reſt.

I might begin with principles drawn from the theory of attraction, which evidently oppoſe every idea of abſolute reſt in any one of the ſtars, when once it is known that ſome of them are in motion: for the change that muſt ariſe by ſuch motion, in the value of a power which acts inverſely as the ſquares of the diſtances, muſt be felt in all the neighbouring ſtars; and if theſe be influenced by the motion of the former, they will again affect thoſe that are next to them, and ſo on till all are in motion. Now as we know ſeveral ſtars, in divers parts of the heavens, do actually change their place, it will follow, that the motion of our ſolar ſyſtem is not a mere hypotheſis; and what will give additional weight to this conſideration is, that we have the greateſt reaſon to ſuppoſe moſt of thoſe very ſtars, which have been obſerved to move, to be

ſuch

ſuch as are neareſt to us; and, therefore, their influence on our ſituation would alone prove a powerful argument in favour of the proper motion of the ſun, had it actually been originally at reſt. But I ſhall wave every view of this ſubject which is not chiefly derived from experience.

To begin with my own, I will give a ſhort but general account of the moſt ſtriking changes I have found to have happened in the heavens ſince FLAMSTEAD's time. I have now almoſt finiſhed my third review. The firſt was made with a Newtonian teleſcope, ſomething leſs than 7 feet focal length, a power of 222, and an aperture of 4¼ inches. It extended only to the ſtars of the firſt, ſecond, third, and fourth magnitudes. Of my ſecond review I have already given ſome account*: it was made with an inſtrument much ſuperior to the former, of 85,2 inches focus, 6,2 inches aperture, and power 227. It extended to all the ſtars in HARRIS's maps, and the teleſcopic ones near them, as far as the eighth magnitude. The catalogue of double ſtars, which I have had the honour of communicating to the Royal Society, and the diſcovery of the Georgium Sidus, were the reſult of that review. My third review was with the ſame inſtrument and aperture, but with a very diſtinct power of 460, which I had already experienced to be much ſuperior to 227, in detecting exceſſively ſmall ſtars, and ſuch as are very cloſe to large ones. At the ſame time I had ready at hand ſmaller powers to be uſed occaſionally after any particularity had been obſerved with the higher powers, in order to ſee the different effects of the ſeveral degrees of magnifying ſuch objects. I had alſo 18 higher magnifiers, which gave me a gradual variety of powers from 460 to upwards of 6000, in order to purſue particular objects to the full extent of

* Phil. Tranſ. vol. LXX. LXXI. LXXII.

my teleſcope, whenever a favourable interval of remarkably fine weather preſented me with a proper opportunity for making uſe of them. This review extended to all the ſtars in FLAMSTEAD's catalogue, together with every ſmall ſtar about them, as far as the tenth, eleventh, or twelfth magnitudes, and occaſionally much farther, to the amount of a great many thouſands of ſtars. To ſhew the practicability of what I have here advanced, it may be proper to mention, that the convenient apparatus of my teleſcope is ſuch, that I have many a night, in the courſe of eleven or twelve hours of obſervation, carefully and ſingly examined not leſs than 400 celeſtial objects, beſides taking meaſures of angles and poſitions of ſome of them with proper micrometers, and ſometimes viewing a particular ſtar for half an hour together, with all the various powers of my teleſcope. The particularities I attended to in this laſt review were, 1. The exiſtence of the ſtar itſelf, ſuch as it is given in the Britiſh catalogue. 2. To obſerve well whether it was double or ſingle, well defined or hazy. 3. To view and mark down its particular colour, whenever the altitude and ſituation of the ſtar would permit it to be done with certainty. 4. To examine all the ſmall ſtars in the neighbourhood, as far at leaſt as the twelfth magnitude, and note the ſame particulars concerning them, except the colours, which would have taken up too much time in committing to paper, and be of no very material uſe. The reſult of theſe obſervations I ſhall collect under a few general heads in the following articles.

I.

Stars that are loſt, or have undergone ſome capital change, ſince FLAMSTEAD's time.

In the Britiſh catalogue we find two remarkable ſtars of the fourth magnitude in the conſtellation of Hercules, *viz.* the

80th

80th and 81ſt. They are no more to be ſeen. I looked for them in October, 1781, but could not find them; and have ſince frequently examined that part of the heavens with no better ſucceſs, though the ſmall ſtars *x*, *z*, *y*, of the ſixth magnitude, not far from the place where the former ſhould be, appear very plainly in a fine evening. On referring to my preceding review in Auguſt, 1780, I find, that I then alſo examined the left foot of Hercules, and had theſe ſtars been there at that time I muſt have ſeen them; for not only *i* of the fourth magnitude, but *y* of the ſixth, is in the liſt of ſtars examined; and the place of the 80th and 81ſt is very near directly between them.

In the northern claw of Cancer FLAMSTEAD has placed three ſtars of the ſixth magnitude; they are the 53d, 55th, and 56th of his catalogue. The latter of them is vaniſhed. Its place and magnitude are ſo well pointed out by the other two ſtars, that there can remain no doubt of this remarkable change. We find a very ſmall teleſcopic ſtar near the place where the 56th ſhould be: this may poſſibly be the remains of that vaniſhing ſtar; but that may be aſcertained by thoſe aſtronomical gentlemen, who, having fixed inſtruments, can determine the place of this ſmall ſtar, and compare it with the 56th of Cancer, when it will appear how far their places agree. I miſſed it firſt in February, 1782, and have ſince looked often for it in vain.

The 19th Perſei, a ſtar of the 6th magnitude, is either loſt, or ſo conſiderably removed from its place in FLAMSTEAD's time, that it is no longer to be known. What gives occaſion for a ſuſpicion of its having been removed is, that we find no ſtar of the ſixth magnitude in the place where this ſhould be; whereas, about a degree following that ſituation, there is one of that, or the fifth magnitude, not taken notice of by FLAM-

STEAD, who could not overlook so considerable a star if it had been there in his time; because, being not far from the parallel of τ and υ, both which he has given us, it must have passed the field of view of his telescope whenever he observed them.

The 108th Piscium, a star of the sixth magnitude, near the head of Aries is lost. The 107th and 109th, both marked as smaller stars in FLAMSTEAD's catalogue, and near the place of the 108th, are easily discovered.

Two stars of the sixth magnitude, the 73d and 74th Cancri, in the southern claw, are either both lost, or at least have undergone such a remarkable change of magnitude, and one of them of place, that it is hardly possible to know them any longer. The alteration must evidently appear when we compare them to the 81st and 82d Cancri; the former of which, though only marked of the 7th magnitude, far outshines the brightest of those which may be supposed to be the two stars in question.

The 8th Hydræ is lost. There is a star just by, which I take to be the 31st Monocerotis. If this should be the 8th Hydræ, and a small star near the latter should agree with the place of th 31st Monocerotis, then the magnitudes will be quite contrary to what FLAMSTEAD makes them. There must, at all events, have been a very remarkable change.

The 26th Cancri is lost. Near this star is placed the 22d of the same constellation, and, as their distance is not much more than a quarter of a degree, it requires fixed instruments to determine which of the two is the star wanting: from the magnitude, however, I surmise, that the remaining star is the 22d rather than the 26th.

The 62d Orionis is lost; and a star near the 54th and 51st is not taken notice of by FLAMSTEAD. Perhaps the 62d has changed place; if this should be the case, it must have a very considerable motion.

The

The 71ſt Herculis, a ſtar of the 5th magnitude, is loſt. The 70th and 71ſt are ſo near each other by FLAMSTEAD's catalogue, that it cannot be determined without fixed inſtruments, which is the ſtar wanting. There is a ſmall teleſcopic ſtar, within about 30 minutes north following, in a direction towards μ Lyræ; if that ſhould be the 71ſt, it is wonderfully changed both in place and ſize. The 40th ſtar in Mr. MAYER's collection of double ſtars * ſeems to be the 70th Herculis of FLAMSTEAD. Now, as that ſtar is perfectly ſingle in my teleſcope, with every power I have tried upon it, we may ſurmiſe that one of the ſtars which is now vaniſhed was ſtill viſible in the year 1778, when Mr. MAYER obſerved it, though then already diminiſhed from the 5th to the 8th magnitude.

The 34th Comæ Berenices is loſt: FLAMSTEAD has marked it as a ſtar of the 5th magnitude.

The 19th of the ſame conſtellation is alſo loſt, or moved and changed in magnitude.

The 40th and 41ſt Draconis have undergone ſo great an alteration of place that we cannot poſſibly miſtake it; for in FLAMSTEAD's time they were above three minutes aſunder, whereas now their diſtance is much leſs than half a minute. A more particular account of theſe two ſtars will be given in a ſecond collection of more than 400 new double ſtars, obſerved in my third review, which I hope ſoon to have the honour of preſenting to the Royal Society.

There ſeems to be an alteration in the place of the 65th, 64th, 54th, and 57th Orionis; but without fixed inſtruments I cannot aſcertain in which of the ſtars it is. Their ſituation in the heaven does not agree with that which is delineated in FLAMSTEAD's Atlas Cœleſtis, for theſe two pair of ſtars are

* De novis in Cœlo ſidereo phænomenis.

much nearer now than they ſhould be, according to that account.

II.

Stars that have changed their magnitude ſince FLAMSTEAD's time.

α Draconis is ſo much leſs than β, which is ſet down as a ſmaller ſtar in FLAMSTEAD's catalogue, that the change of magnitude cannot be doubted.

β Ceti marked of the 3d, and α Ceti of the 2d, are evidently the reverſe, β being by much the larger ſtar. I have mentioned this circumſtance in my obſervations on the periodical ſtar in Collo Ceti *, and it ſeems now as if the difference between the magnitudes of theſe two ſtars was ſtill increaſing.

ζ Serpentis is not near ſo large as η, and yet we find FLAMSTEAD has placed them in the ſame claſs: however, we cannot intirely confide in the marks of the magnitudes when two ſtars are placed in the ſame claſs, ſince every order admits of a conſiderable variety; but when the marks contradict experience ſo far as to deſcribe one ſtar, for inſtance, of the third, and another of the 4th magnitude, when obſervation ſhews the latter to be of the 3d and the former of the 4th, I think we can hardly doubt but that there muſt have been a change.

η Cygni is a brighter ſtar than χ, though marked by FLAMSTEAD of a leſs magnitude.

The 2d Urſæ minoris is marked of the 6th magnitude, but is certainly intitled to the 5th.

η Bootis is much larger than ζ.

ι Delphini is much larger than κ.

β Trianguli is much larger than α.

γ Aquilæ is much larger than β.

* Phil. Tranſ. vol. LXX. numb. xxi.

σ Sagittarii is larger than δ, γ, and ϵ, though marked of an inferior magnitude.

δ Canis majoris is larger than β, and yet is marked to be leſs.

η Serpentis is ſo much larger than ζ, that they certainly ſhould not have been put in the ſame order of magnitude.

κ Serpentarii is larger than γ and ϵ, though marked to be of a leſs magnitude than either.

β Equulei is ſo much leſs than α that it could hardly deſerve to be put in the ſame claſs.

δ Delphini is larger than ϵ, though placed in an inferior order.

ϵ Bootis is ſo much larger than ζ that it ſhould not be put into the ſame order.

δ Sagittæ is larger than α and β, though placed in a lower order of magnitude.

δ Urſæ majoris is leſs than either ϵ, ζ, or η, though it is marked of a ſuperior order of magnitude. Beſides, it is evidently viſible, that δ cannot be intitled to more than the 4th magnitude, or at moſt to between the 4th and 3d: on the contrary, ϵ, ζ and η, ſhould be of the 2d, or at leaſt between the 2d and 3d; all which is very different from FLAMSTEAD's account of thoſe remarkable ſtars.

α Urſæ majoris is leſs than any ſtar marked of the ſame magnitude, and cannot have the leaſt pretenſion to be called a ſtar of between the 1ſt and 2d, as FLAMSTEAD has marked it, and as I make no doubt it was in his time.

The 1ſt and 2d Hydræ are noted by FLAMSTEAD as being of the 4th magnitude, whereas they now are only of the 8th or 9th. It is remarkable, that the 30th Monocerotis, which is ſituated between them, has retained the order aſſigned to it

by FLAMSTEAD, and being of the 6th ſerves to point out the change of the other two in a very evident manner.

γ Lyræ is much larger than β.

The change in the magnitudes of the 31ſt and 34th Draconis is very ſtriking, theſe two ſtars being juſt the contrary of what they are marked in FLAMSTEAD's catalogue. The 31ſt from the 7th is increaſed to the 4th; and the 34th, from being a ſtar between the 4th and 5th, is reduced to one of the 6th, if not 7th magnitude.

The 44th Cancri is much too ſmall for the 6th magnitude. As ϵ and others are marked of the 6th, this, on being compared to them, can be intitled to no more than the 8th or 9th order.

The 96th Tauri is ſmall enough to be of the 8th magnitude, though marked as one of the 6th.

The 62d Arietis is of the 5th magnitude, though only marked of the 6th.

The magnitudes of the 12th and 14th Lyncis are juſt the reverſe in the heavens to what FLAMSTEAD has marked them. This denotes a double change of a ſtar from the 5th to the 7th, and from the 7th to the 5th magnitude.

The 38th Perſei, marked of the 6th magnitude, is increaſed ſo as to be equal to θ and $\varkappa$ of the 4th. Alſo, θ is leſs than τ contrary to FLAMSTEAD.

The 8th Monocerotis is leſs than the 76th Orionis, though the former ſhould be of the 4th, and the latter only of the 6th magnitude.

The 23d Geminorum, though marked of the 5th, is leſs than the 21ſt of between the 6th and 7th magnitude.

The

The 26th Orionis is much too ſmall for the magnitude of which it is marked to be, or rather is loſt; for I can hardly take any one of the remaining teleſcopic ſtars for it.

ξ Leonis in FLAMSTEAD's time was of the 4th; but is now leſs than a ſtar of the 5th magnitude.

III.

Stars newly come to be viſible.

Near Lacerta's tail-end is a ſtar of between the 4th and 5th magnitude, not mentioned in FLAMSTEAD's catalogue, though the 1ſt Lacertæ, not far from that place, is recorded. It is ſo eaſy to be ſeen with the naked eye, and in a ſpot where but few ſtars of that magnitude are near, that we can hardly account for its being omitted if it had been viſible to FLAMSTEAD. Its colour is pale red.

The ſtar of the 5th magnitude following τ Perſei, ſuppoſed to be υ removed, is moſt likely new, unleſs future obſervations were to favour the ſuppoſed motion of this ſtar. It is among the double ſtars of my 4th claſs, ſo that it will be eaſy to detect its proper motion.

A very conſiderable ſtar, not marked by FLAMSTEAD, will be found near the head of Cepheus. Its right aſcenſion in time, is about 2′ 19″ preceding FLAMSTEAD's 10th Cephei, and it is about 2° 20′ 3″ more ſouth than the ſame ſtar. It is of a very fine deep garnet colour, ſuch as the periodical ſtar ο Ceti was formerly, and a moſt beautiful object, eſpecially if we look for ſome time at a white ſtar before we turn our teleſcope to it, ſuch as α Cephei, which is near at hand.

A conſiderable ſtar in a direction from the 68th Geminorum towards the 61ſt is not to be found in FLAMSTEAD, its colour is red.

A ſtar of a conſiderable magnitude preceding the 1ſt Equulei is not contained in FLAMSTEAD's catalogue. It is a double ſtar of the firſt claſs, the 61ſt of my ſecond collection, where meaſures of it will be found.

A conſiderable ſtar following the 1ſt Sextantis, and another following the 7th, are not inſerted.

Between β Cancri and δ Hydræ is a very conſiderable ſtar not marked by FLAMSTEAD, though its ſituation is very remarkable. As the conſtellation of Cancer contains ſo rich a collection of very ſmall ſtars, it is to be wondered how a ſtar of ſuch conſequence could be omitted, if it had been viſible in FLAMSTEAD's time.

Nearly 1½ degree north following δ Herculis, almoſt in the direction of δ and ν, is a ſtar of the 5th, or between the 4th and 5th magnitude, very viſible to the naked eye. We can hardly think FLAMSTEAD could have overlooked it, had it been there in his time.

About 3 degrees ſouth preceding γ Bootis, a conſiderable ſtar not in FLAMSTEAD's catalogue of the 6th magnitude; and ſouth preceding λ, another, almoſt as large.

Here we ought to obſerve, that it is not eaſy to prove a ſtar to be newly come; for though it ſhould not be contained in any catalogue whatſoever, yet the argument for its former non-appearance, which is taken from its not having been obſerved, is only ſo far to be regarded as it can be made probable, or almoſt certain, that a ſtar would have been obſerved had it been viſible. For theſe reaſons I will lay no particular ſtreſs on the new appearance of the above ſtars; they are, however, ſuch as do well deſerve to have their places ſettled, while I ſhall leave it to others to determine how far they may think them to be new viſitors to thoſe ſtarry regions that fall within the reach of our ſight.

To

To return to the principal ſubject of this paper, which is the proper motion of the ſun and ſolar ſyſtem: does it not ſeem very natural, that ſo many changes among the ſtars,—many increaſing their magnitude, while numbers ſeem gradually to vaniſh;—ſeveral of them ſtrongly ſuſpected to be new-comers, while we are ſure that others are loſt out of our ſight;—the diſtance of many actually changing, while many more are ſuſpected to have a conſiderable motion:—I ſay, does it not ſeem natural that theſe obſervations ſhould cauſe a ſtrong ſuſpicion that moſt probably every ſtar in the heaven is more or leſs in motion? And though we have no reaſon to think, that the diſappearance of ſome ſtars, or new appearance of others, nor indeed the frequent changes in the magnitudes of ſo many of them are owing to their change of diſtance from us by proper motions, which could not occaſion theſe phenomena without being inconceivably quick; yet we may well ſuppoſe, that motion is ſome way or other concerned in producing theſe effects. A ſlow motion, for inſtance, in an orbit round ſome large opaque body, where the ſtar, which is loſt or diminiſhed in magnitude, might undergo occaſional occultations, would account for ſome of thoſe changes, while others might perhaps be owing to the periodical return of large ſpots on that ſide of the ſurface which is alternately turned towards us by a rotatory motion of the ſtar. The idea alſo of a body much flattened by a quick rotation, and having a motion ſimilar to the moon's orbit by a change of the place of its nodes, whereby more of the luminous ſurface would one time be expoſed to us than another, tends to the ſame end; for we cannot help thinking with Mr. DE LA LANDE (Mem. 1776), that the ſame force which gave ſuch rotations, would probably alſo

alſo occaſion motions of a different kind by a tranſlation of the center *. Now, if the proper motion of the ſtars in general be once admitted, who can refuſe to allow that our ſun, with all its planets and comets, that is, the ſolar ſyſtem, is no leſs liable to ſuch a general agitation as we find to obtain among all the reſt of the celeſtial bodies †.

Admitting this for granted, the greateſt difficulty will be how to diſcern the proper motion of the ſun between ſo many other (and variouſly compounded) motions of the ſtars. This is an arduous taſk indeed, which we muſt not hope to ſee accompliſhed in a little time; but we are not to be diſcouraged from the attempt. Let us, at all events, endeavour to lay a good foundation for thoſe who are to come after us. I ſhall therefore now point out the method of detecting the direction and quantity of the ſuppoſed proper motion of the ſun by a few geometrical deductions, and at the ſame time ſhew by an application of them to ſome known facts, that we have already ſome reaſons to gueſs which way the ſolar ſyſtem is probably tending its courſe.

Suppoſe the ſun to be at S, fig. 1.; the fixed ſtars to be diſperſed in all poſſible directions and diſtances around at *s*, *s*, *s*, *s*, &c. Now, ſetting aſide the proper motion of the ſtars, let us firſt conſider what will be the conſequence of a proper motion in the ſun; and let it move in a direction from A towards B.

* Relating to the motion of the fixed ſtars, the Aſtronomer Royal has an expreſſion in the ſecond page of the explanation and uſe of the tables publiſhed in his Aſtronomical Obſervations, which ſeems to favour this idea, where he mentions the "peculiar but ſmall motions, which many, IF NOT ALL OF THEM, "have among themſelves, which have been called their *proper motions*, the cauſes "and laws of which are hid for the preſent in almoſt equal obſcurity."

† See Mr. MICHELL's note, Phil. Tranſ. vol. LVII. p. 252.

Suppoſe it now arrived at C. Here, by a mere inſpection of the figure, it will be evident, that the ſtars *s*, *s*, *s*, which were before ſeen at *a*, *a*, *a*, will now, by the motion of the ſun from S to C, appear to have gone in a contrary direction, and be ſeen at *b*, *b*, *b*; that is to ſay, every ſtar will appear more or leſs to have receded from the point B, in the order of the letters *ab*, *ab*, *ab*. The converſe of this propoſition is equally true; for if the ſtars ſhould all appear to have had a retrograde motion, with reſpect to the point B, it is plain, on a ſuppoſition of their being at reſt, the ſun muſt have a direct motion towards the point B, to occaſion all theſe appearances. From a due conſideration of what has been ſaid, we may draw the following inferences.

1. The greateſt or total ſyſtematical parallax of the fixed ſtars, fig. 2. will fall upon thoſe that are in the line DE, at rectangles to the direction AB of the ſun's motion.

2. The partial ſyſtematical parallax of every other ſtar, *s*, *s*, *s*, not in the line DE, will be to the total parallax as the ſine of the angle BS*a*, being the ſtars diſtance from that point towards which the ſun moves, to radius.

3. The parallax of ſtars at different diſtances will be inverſely as thoſe diſtances; that is, one half at double the diſtance, one third at three times, and ſo on; for the ſubtenſe SC remaining the ſame, and the parallactic angle being very ſmall, we may admit the angle S*s*C, to be inverſely as the ſide S*s*, which is the ſtars diſtance.

4. Every ſtar at reſt, to a ſyſtem in motion, will appear to move in a direction contrary to that in which the ſyſtem is moving.

Corollary. Hence it follows, that if the ſolar ſyſtem be carried towards any ſtar ſituated in the ecliptic: every ſtar, whoſe

angular

angular diſtance *in antecedentia* (reckoned upon the ecliptic from the ſtar towards which the ſyſtem moves) is leſs than 180 degrees, will decreaſe in longitude. And that, on the contrary, every ſtar, whoſe diſtance from the ſame ſtar (reckoned upon the ecliptic but *in conſequentia*) is leſs than 180 degrees, will increaſe in longitude, in both caſes without alteration of latitude.

From theſe principles it would be eaſy to draw general theorems for every poſſible direction of the motion of the ſolar ſyſtem, by which we might find what alteration of longitude or latitude would take place in any given ſtar; but it will be time enough for thoſe inveſtigations when we ſhall have more immediate occaſion for them. What we are now chiefly to endeavour at is, the ſpeedy method of obtaining ſufficient facts to proceed upon.

The immenſe regions of the fixed ſtars may be conſidered as an infinitely expanded globe, having the ſolar ſyſtem for its center. With this idea it will occur to us, that no method can be ſo proper for finding out the direction of the motion of the ſun as to divide our obſervations on the ſyſtematical parallax of the fixed ſtars into three principal zones. Theſe, for the convenience of fixed inſtruments, may be aſſumed ſo as to let them paſs around the equator and the equinoctial and ſolſtitial colures, every one being at rectangles to the other two, according to the three dimenſions of ſolids. And ſince no obſervations can be ſo conveniently made to aſcertain ſmall relative proper motions among the fixed ſtars as thoſe on double ſtars, I have continued my reſearches in that line with great application, and can now furniſh out theſe three zones, with a very complete ſet of double ſtars for ſuch obſervations. We have the greateſt reaſon to hope for ſucceſs in this attempt; for,

if

if I am not mistaken, there will be found a secular systematical parallax of some considerable value; nay, possibly, so short a space of time as ten years may suffice to bring us acquainted with many hitherto unknown celestial motions.

The equatorial zone, extending 10 degrees on each side of the equator, will contain about 150 stars which I have found to be double, *viz.*

Piscium 38. 51. 77. 86. north of 110. 113.

Ceti south of 13. foll. 25. 26. 37. north of 37. foll. 54. 61. 66.

Eridani 32. near 48. 62. 69.

Tauri near 10. 45. foll. 66. 88.

Orionis foll. 1. near 10. foll. 10. 19. pre. 20. 20. 23. near 26. 28. near 28. pre. 29. near 30. between 30 and 33. 32. 33. 34. foll. 34. pre. 36. 39. 41. near 42. 44. another 44. pre. 46. north of 46. foll. 47. 48. 50. 52. pre. 58. 58. 59.

Monocerotis 8. 11. near 11. In naribus. Sub genam. Inter pedes. near 12. foll. 15. pre. 25. foll. 25. 29. 31. near 31.

Canis minoris near 10. foll. 10. 14.

Cancri 17.

Hydræ pre. 4. pre. 4. foll. 4. 15. 17. pre. 22. 22. foll. 22. 27. 30. 31.

Leonis 3. foll. 3. pre. 43. south of 43. 57. foll. 63. 74. pre. 75. 83. 84.

Virginis 4. 17. 25. 29. pre. 44. 44. 51. 51. 84. pre. 93. 93.

Libræ 17. near 31.

Serpentis 58. 59. 63.

Serpentarii near 11. 53. 61. 67. south of 67. 70.

Aquilæ 2. near 6. near 6. pre. 7 and 8. 15. 24. pre. 30 near 35. near 35. 49. 53. near 54. 57. near 63. 64. near 65. near 65.

Delphini 1.

Aquarii 4. 4. 22. 24. 51. 55. ſouth of 72.

Equulei 1. pre. 1. ſouth of 2. ſouth of 6. 7.

Pegaſi 3. near 3. 8. near 18.

Piſcium near 7. 8. ſouth of 10. 35.

Ceti ſouth of 4.

The zone of the equinoctial colure, extending 10 degrees of a great circle on each ſide, will contain, as far as it is viſible in our hemiſphere, about 70 double ſtars, *viz.*

Ceti foll. 4. near 13.

Aquarii 107.

Piſcium 51. 38. 35. 76.

Andromedæ 21. Supra caput. 29. pre. 23. near 27. near 16. near 17.

Caſſiopeæ pre. 25. prec. 25. 8. 24. 18. 6. 9. ſouth of 11. 55. 34. 31. 4. 3. 2. near 33. 35. 36. near 3. 44. 47.

Cephei 1. foll. 32. foll. 32. foll. 31.

Urſæ minoris 1. 18.

Draconis 40 and 41. between 10 and 11. near 77.

Urſæ majoris 79. between 50 and 38. foll. 42. 65. 57. near 58. ſouth of 69.

Canum venaticorum 12. 2.

Comæ Berenices 24. from 36 to 26. 2. 12.

Leonis 95. 90. 93. pre. 95.

Virginis 29. pre. 44. between 4 and 6. 17. 25. 27. ſouth of 12. 4.

Corvi 7.

Crateris 17. foll. 21.

The

The zone of the ſolſtitial colure, of the ſame extent, will include about 120 double ſtars, *viz.*

Canis majoris north of 13. 17. foll. 5. foll 2. foll. 5.

Monocerotis 11. 8. 12. near 11. In naribus. foll. 15. Inter pedes. Sub genam.

Leporis 13.

Orionis 48. near 42. 41. 44. 44. 50. foll. 72. 58. In fuſte. 59. pre. 70 and 76. pre. 58. pre. 69.

Geminorum 43. 38. pre. 1. between 13 and 18. 21. 15. north of 19. 12. near 34. 27. 37. near 24. near 24. ſouth of 18. foll. 13 and 18.

Tauri north of 123.

Aurigæ 29. 13. 37. 34. 32. ſouth of 34. 56. near 59. pre. 58. pre. Nebulam. 41.

Lyncis 12. In naribus. 5. 13. In pectore. 19.

Camelopardali. In aure.

Urſæ minoris 1. near 18. near 12.

Draconis 39. 63. 19. 21. 56. 31. 47. 24 and 25. 69. 46. near 31. 40 and 41. 48. near 23. near 77.

Cephei 1. 8.

Herculis 75. 95. over 85. near 87. 100. 86. near 94. near 103.

Lyræ 4. 5. 6. 10. 3. 8. 11. near 3. near 6 and 7. foll. 5. foll. 10. pre. 10. pre. 14 and 15.

Serpentarii 70. 54. 61. 53. 67. near 67. north of 72. north of 72.

Serpentis 59. 63. 58.

Aquilæ near 7 and 8. 2. near 6. near 6.

Sagittarii 13. 38.

It will not be amiſs to add a zone of the ecliptic, which will contain, among others, a great many double ſtars that may

undergo occultations by the moon or planets. This is of the ſame extent, and includes about 120 double ſtars.

Arietis north of 3. 5. pre. 6. north of 6. pre. 17. 30. 42. pre. 54. foll. 62. pre. 63. foll. 63. foll. 63. foll. 63.

Tauri near 4. 7. pre. 8. foll. 11. 30. 52. 59. 62. 68. foll. 68. pre. 74. 87. 94. 103. north of 103. 105. 111. foll. 112. 114. foll. 117. 118. north of 123. *x*.

Orionis 68. pre. 69. pre. 70 and 76.

Aurigæ 14. 26. pre. Nebulam.

Geminorum pre. 1. 4. 12. foll. 13 and 18. 15. 18. 21. pre. 24. near 24. near 24. 27. 37. 38. 54. foll. 55. pre. 61. 63. north of 63. 66. 78. foll. 78. foll. 81.

Cancri 11. 16. foll. 16. 22. 23. 24. 30. 48. 54. pre. 77. *ω* foll. *ι*.

Leonis 2. 3. foll. 3. 6. 7. 14. 25. 32. 41. ſouth of 43. pre. 43. foll. 44. 57. foll. 63. 74. pre. 75. 83. 84.

Virginis 4. foll. 4. pre. 4. 17. 25. 44. 51.

Hydræ 54.

Libræ pre. 12. 18. 24. 31. 51.

Scorpii 8. 14.

Serpentarii 5. 38. 39.

Sagittarii 13. 38. foll. 43. 64. near 65.

Capricorni north of 1. 5. 7. 9. 11. 12. near 29. 29. 39.

Aquarii 14. 22. foll. 43. 51. 55. 57. 70. 71. 72. 91. 94.

Piſcium near 7. ſouth of 8. 35. 38. 51. 77. 86. 110. 113.

Ceti foll. 4. near 13. 26. 54.

An account of each of theſe double ſtars, not already in my firſt catalogue, will be contained in the ſecond collection. It remains now only for me to make an application of this theory to ſome of the facts we are already acquainted with, relating to the proper motion of the ſtars. And firſt let me obſerve, that the

the rules of philoſophizing direct us to refer all phenomena to as few and ſimple principles as are ſufficient to explain them. Thus, for inſtance, we ſee the ſtars and planets riſe and ſet every day: now, as it is much more ſimple to admit the earth to turn once in 24 hours, than to ſuppoſe every ſingle ſtar to revolve round the earth in that time, we very juſtly aſcribe a diurnal motion to the earth; but yet, ſince we find that the planets do not every night exactly retain their relative places among the ſtars, we next admit that ſuch deviations from the law, which all the reſt ſeem to obey, are owing to a proper motion of their own. To apply this to the ſolar ſyſtem.—Aſtronomers have already obſerved what they call a proper motion in ſeveral of the fixed ſtars, and the ſame may be ſuppoſed of them all. We ought, therefore, to reſolve that which is common to all the ſtars, which are found to have what has been called a proper motion, into a ſingle real motion of the ſolar ſyſtem, as far as that will anſwer the known facts, and only to attribute to the proper motion of each particular ſtar the deviations from the general law the ſtars ſeem to follow in thoſe movements.

By Dr. MASKELYNE's account of the proper motion of ſome principal ſtars*, we find that Sirius, Caſtor, Procyon, Pollux, Regulus, Arcturus, and α Aquilæ, appear to have reſpectively the following proper motions in right aſcenſion. −0″,63; −0″,28; −0″,80; −0″,93; −0″,41; −1″,40; and +0″,57; and two of them, Sirius and Arcturus, in declination, *viz.* 1′,20 and 2″,01, both ſouthward. Let fig. 3. repreſent an equatorial zone, with the above mentioned ſtars referred to it, according to their reſpective right aſcenſions,

* Aſtronomical Obſervations made at the Royal Obſervatory at Greenwich.

having the ſolar ſyſtem in its center. Aſſume the direction AB from a point ſomewhere not far from the 77th degree of right aſcenſion to its oppoſite 257th degree, and ſuppoſe the ſun to move in that direction from S towards B; then will that one motion anſwer that of all the ſtars together: for if the ſuppoſition be true, Arcturus, Regulus, Pollux, Procyon, Caſtor, and Sirius, ſhould appear to decreaſe in right aſcenſion, while α Aquilæ, on the contrary, ſhould appear to increaſe. Moreover, ſuppoſe the ſun to aſcend at the ſame time in the ſame direction towards ſome point in the northern hemiſphere, for inſtance, towards the conſtellation of Hercules; then will alſo the obſerved change of declination of Sirius and Arcturus be reſolved into the ſingle motion of the ſolar ſyſtem. I am well aware of the many yet remaining difficulties, ſuch as the correſpondence of the exact quantity of each ſtar's obſerved proper motion with the quantity that will be aſſigned to it by this hypotheſis; but we ought to remember, that the very different and ſtill unknown relative diſtances of the fixed ſtars muſt, for a good while yet, leave us in the dark about the particular and ſtrict application of the theory; and that any deviation from it may eaſily be accounted for by the ſtill unknown *real* proper motion of the ſtars: for if the ſolar ſyſtem have the motion I aſcribe to it, then what aſtronomers have already obſerved concerning the change of place of the ſtars, and have called their proper motion, will become only an *apparent* motion; and it will ſtill be left to future obſervations to point out, by the deviations from the general law which the ſtars will follow in thoſe apparent motions, what may be their real proper motions as well as relative diſtances. But leſt I ſhould be cenſured for admitting ſo new and capital a motion upon too ſlight a foun-

dation, I must observe, that the concurrence of those seven principal stars cannot but give some value to an hypothesis that will simplify the celestial motions in general. We know that the sun, at the distance of a fixed star, would apppear like one of them; and from analogy we conclude the stars to be suns. Now, since the apparent motions of these seven stars may be accounted for, either by supposing them to move just in the manner they appear to do, or else by supposing the sun alone to have a motion in a direction, somehow not far from that which I have assigned to it, I think we are no more authorised to suppose the sun at rest than we should be to deny the diurnal motion of the earth, except in this respect, that the proofs of the latter are very numerous, whereas the former rests only on a few though capital testimonies. But to proceed: I have only mentioned the motions of those seven principal stars, as being the most noticed and best ascertained of all; I will now adduce a farther confirmation of the same from other stars.

M. DE LA LANDE gives us the following table of the proper motion of 12 stars, both in right ascension and declination, in 50 years *.

* Ast. par M. DE LA LANDE, tom. IV. p. 685.

Etoiles.	Chang. d'ate. droite.	Chang. de déclinaiſon.
	′ ″	′ ″
Arcturus	− 1 11	− 1 55
Sirius	− 37	− 52
β Cygni	− 3	+ 49
Procyon	− 33	− 47
ε Cygni	+ 20	+ 34
γ Arietis	− 14	− 29
γ Gemin.	− 8	− 24
Aldébaran	+ 3	− 18
β Gemin.	− 48	− 16
γ Piſcium	+ 53	+ 7
α Aquilæ	+ 32	− 4
α Gemin.	− 24	− 1

Fig. 4. repreſents them projected on the plane of the equator. They are all in the northern hemiſphere, except Sirius, which muſt be ſuppoſed to be viewed in the concave part of the oppoſite half of the globe, while the reſt are drawn on the convex ſurface. Regulus being added to that number, and Caſtor being double, we have 14 ſtars. Every ſtar's motion, except Regulus, is aſſigned in declination as well as in right aſcenſion, ſo that we have no leſs than 27 motions given to account for. Now, by aſſuming a point ſomewhere near λ Herculis, and ſuppoſing the ſun to have a proper motion towards that part of the heaven, we ſhall ſatisfy 22 of theſe motions. For β Cygni, α Aquilæ, ε Cygni, γ Piſcium, γ Arietis, and Aldebaran, ought, upon the ſuppoſed motion of the ſun, to have an apparent progreſſion, according to the hour circle XVIII, XIX, XX, &c. or to increaſe in right aſcenſion, while Arcturus, Regulus, the two ſtars of α Geminorum, Pollux, Procyon, Sirius, and γ Geminorum, ſhould apparently

go

go back in the order XVI, XV, XIV, &c. of the hour circle, ſo as to decreaſe in right aſcenſion; but according to M. DE LA LANDE's table, excepting β Cygni and γ Arietis, all theſe motions really take place. With regard to the change of declination, we ſee that every ſtar in the table ſhould go towards the ſouth; and here we find but three exceptions in β and ε Cygni, and γ Piſcium; ſo that upon the whole we have but five deviations out of 27 known motions which this hypotheſis will not account for. And theſe exceptions muſt be reſolved into the real proper motion of the ſtars.

There are alſo ſome very ſtriking circumſtances in the quantities of theſe motions that deſerve our notice. Firſt, Arcturus and Sirius being the largeſt of the ſtars, and therefore probably the neareſt, ought to have the moſt apparent motion, both in right aſcenſion and declination, which is agreeable to obſervation, as we find by the table. Next, in regard to the right aſcenſion only, Arcturus being better ſituated to ſhew its motion, by theorem II. p. 261. ought to have it much larger, which we find it has. Aldebaran, both badly ſituated and conſiderably ſmaller than the two former, by the ſame theorem ought to ſhew but little motion. Procyon, better ſituated than Sirius, though not quite ſo large, ſhould have almoſt as much motion; for by the third theorem, on ſuppoſing it farther off becauſe it appears ſmaller, the effect of the ſun's motion will be leſſened upon it; whereas, on the other hand, by the ſecond theorem, its better ſituation will partly compenſate for its greater diſtance. This again is conformable to the table. ε Cygni very favourably ſituated, though but a ſmall ſtar, ſhould ſhew it conſiderably as well as α Aquilæ; whereas β Cygni ſhould have but little motion: and γ Piſcium, beſt

ſituated of all, ſhould have a great increaſe of right aſcenſion, and theſe deductions alſo agree with the table.

In the laſt place, a very ſtriking agreement with the hypotheſis is diſplayed in Caſtor and Pollux. They are both pretty well ſituated, and we accordingly find that Pollux, for the ſize of the ſtar, ſhews as much motion in right aſcenſion as we could expect; but it is remarkable, and ſeemingly contrary to our hypotheſis, that Caſtor, equally well placed, ſhews by the table no more than one half of the motion of Pollux. Now, if we recollect that the former is a double ſtar, conſiſting of two ſtars not much different in ſize, we can allow but about half the light to each of them, which affords a ſtrong preſumption of their being at a greater diſtance, and therefore their partial ſyſtematical parallax, by the third theorem, ought to be ſo much leſs than that of Pollux, which agrees wonderfully with obſervation *. Not to mention the great difficulty in which we ſhould be involved, were we to ſuppoſe the motion of Caſtor to be really in the ſtar: for how extraordinary muſt appear the concurrence, that two ſtars, namely thoſe that make up this apparently ſingle ſtar, ſhould both have a proper motion ſo exactly alike, that in all our obſervations hitherto they have not been found to diſagree a ſingle ſecond, either in right aſcenſion or declination, for fifty years together! Does not this ſeem ſtrongly to point out the common cauſe, the motion of the ſolar ſyſtem?

* If the light of Caſtor was exactly equal to that of Pollux, and the two ſtars, which make up the former ſtar, were perfectly of the ſame ſize, we might, on that account, ſuppoſe the diſtance of Caſtor from us to be to that of Pollux as $\sqrt{2} : 1$; but Caſtor is in fact ſomething leſs bright; and this conſideration, added to the former, will make it probable enough that its diſtance may perhaps be double that of Pollux.

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With respect to the change of declination I would observe, that the point of λ Herculis, which in fig. 4. is assumed as the Apex * of the solar motion is not perhaps the best selected. A somewhat more northern situation may agree better with the changes of declination of Arcturus and Sirius, which capital stars may perhaps be the most proper to lead us in this hypothesis; but as we should be guided by facts in researches of this nature, it may be as well to expect the assistance of future observations before we are too particular in determining this point †.

It may be expected I should also mention something concerning the quantity of the solar motion; but here I can only offer a few distant hints. From the annual parallax of the fixed stars, which, from my own observations, I find much less than it has hitherto been proved to be, we may certainly admit (without entering into a subject which I reserve for a future opportunity) that the diameter of the earth's orbit, at the distance of Sirius or Arcturus, would not nearly subtend an angle of one second; but the apparent motion of Arcturus, if owing to a translation of the solar system, amounts to no less than 2″,7 a year, as will appear if we compound the two motions of 1′ 11″ in right ascension, and 1′ 55″ in declination, into one single motion, and reduce it to an annual quantity.

* I use the term Apex here to denote that point of fig. 4. wherein all great circles, drawn through the supposed direction of the motion of the solar system, intersect, and which, in other stereographic projections, is generally a pole, either of the ecliptic or equator. As this point is in the northern or elevated hemisphere, the sun, by tending to it, may be said to ascend, and the term Apex may perhaps not be an improper one.

† From the additional testimony of other capital stars considered in the postscript it now appears, that the point of λ Herculis is probably as well chosen as any we can fix upon in that part of the heavens.

Hence we may in a general way eſtimate, that the ſolar motion can certainly not be leſs than that which the earth has in her annual orbit.

I have now only to add, that it is to be expected future obſervations will ſoon throw more light upon this intereſting ſubject, and either fully eſtabliſh or overturn the hypotheſis of the motion of the whole ſolar ſyſtem. To this end I have already begun a ſeries of obſervation upon ſeveral zones of double ſtars; and ſhould the reſult of them be againſt theſe conjectures, I ſhall be the firſt to endeavour to point out the fallacy of them.

Datchet near Windſor,
Feb. 1, 1783.

Poſtſcript to the Paper on the Motion of the Solar Syſtem.

In my paper on the Motion of the Solar Syſtem, I uſed a table of the proper motion of ſome fixed ſtars, which M. DE LA LANDE has given us as an extract from TOB. MAYER's Opera inedita. By the favour of my aſtronomical friend Mr. AUBERT, I am now furniſhed with the ſcarce edition of the original. This work contains a catalogue of the place of 80 ſtars, obſerved by Mr. MAYER in 1756, and compared with the ſame ſtars as given by ROEMER in 1706. From the goodneſs of the inſtrument with which the obſervations to which Mr. MAYER has compared his own were made, he gives it as his opinion, that where the diſagreement in the place of a ſtar is but ſmall, it may

may be attributed to the imperfection of the instrument; but that when it amounts to 10 or 15″, it is a very probable indication of a proper motion of such a star. He adds, that when the disagreement is so much as in some stars which he names, (among which is FOMAHAND, where the difference is 21″ in 50 years) he has not the least doubt of a proper motion *.

By this extensive table I thought it highly necessary immediately to examine the hypothesis of the motion of the solar system, that it might receive an early check from observations, if they should be unfavourable; or that, on the other hand, it might be supported by the additional evidence of more stars, if their apparent proper motions should coincide with the idea I have pointed out in my paper on this subject.

I have followed Mr. MAYER's judgement of his own and ROEMER's observations, and left out of the list all the stars that do not shew a disagreement amounting to 10″ in the places which are given for them in 1706 and 1756. I have also left out those 13, or rather 14 stars, which have already been examined in my paper, and have been shewn to support the hypothesis I have advanced: the rest are here drawn up in two tables. The first contains the stars that agree with my assigned motion of the solar system; or rather which are thereby resolved into apparent, or partly apparent, and partly proper motions. The second table contains those stars whose motions cannot be accounted for by my hypothesis, and must therefore be ascribed to a real motion in the stars themselves, or to some *still more hidden* cause of a *still remoter* parallax †.

* De motu fixarum proprio Commentatio. Op. ined. Vol. I. p. 79.

† That I may not be obscure, it will be proper to mention what I allude to, especially as it claims a distant connection with our subject, and may hereafter become of sufficient moment to engage our attention. Mr. MICHELL's admirable idea

idea of the ſtars being collected into ſyſtems (Phil. Tranſ. vol. LI, p. 249.) appears to be extremely well-founded, and is every day more confirmed by obſervations: though this does not, in my opinion, take away the probability of many ſtars being ſtill as it were *ſolitary*, or, if I may uſe the expreſſion, *interſyſtematical*. It occurs then naturally, that by the principle of gravitation, which is never at reſt, and which we have no reaſon to doubt extends to all poſſible diſtances, one ſyſtem of ſtars will act on another as if the ſtars of each ſyſtem were all collected into the center of gravity of each reſpective ſyſtem. Hence then will ariſe this evident conſequence, that a ſtar, or ſun, ſuch as ours, may have a proper motion within its own ſyſtem of ſtars, while at the ſame time the whole ſtarry ſyſtem to which it belongs may have another proper motion, totally different in quantity and direction. It will require no little abſtract conſideration to conceive the poſſibility of what may be thus ſurmiſed; therefore an inſtance or two, to elucidate the matter, may not be improper. If an inhabitant of the 5th ſatellite of Saturn ſhould have diſcovered, that his little world revolves at a great diſtance round a planet, and to his great aſtoniſhment ſhould alſo have found, that this planet again revolves round the ſun;—if, farther, our hypotheſis of the ſolar motion ſhould prove to be well-founded (which, in ſome of the ſtars, ſuppoſing them to be ſuns ſurrounded with planets and ſatellites, muſt certainly be the caſe); then a third capital motion will be introduced to this inhabitant of Saturn's ſatellite; and he will experience, in a narrow compaſs, what we now ſurmiſe may poſſibly be our caſe upon a more extended ſcale, by the motion of the whole ſyſtem of ſtars to which our ſun may belong. Another view may, perhaps, ſtill better throw a light upon the ſubject. Let us admit that a very ſmall nebula may be a collection of a thouſand ſtars: and if Mr. MICHELL's opinion of our ſyſtem of ſtars, which he aſſumes to be about a thouſand (Phil. Tranſ. vol. LVII. p. 255.) has any foundation, all theſe ſtars taken together will only ſubtend an angle of barely a minute to an eye placed 3438 times as far from the center of the ſyſtem as the two fartheſt ſtars in it are from each other. Now as I have found ſome of theſe nebulæ that are ſo ſmall, that a tolerably good teleſcope cannot diſtinguiſh them from a ſingle ſtar, whole ſyſtems of ſtars, when preſented to our imagination under this diminutive ſhape of nebulæ, will eaſily, I believe, be admitted among the number of thoſe celeſtial bodies that may have a proper motion. I ought to carry this hint a little farther, juſt to ſhew that it may poſſibly be applied to the ſubject of reſolving a number of concurrent proper motions of the fixed ſtars into apparent ones; and thereby, in proceſs of time, to arrive at the knowledge of all the real complicated motions of the planet we inhabit; of the ſolar ſyſtem

to

TABLE I.

Names of ſtars.	Motion in R.A.	Motion in Decl.	Names of ſtars.	Motion in R.A.	Motion in Decl.
β Ceti	+32		ζ Hydræ	−23	
α Arietis	+10		γ Leporis		−10
δ Ceti	+15		ε Urſæ majoris	−33	+10
α Ceti	+16		α Serpentarii	inſenſ.	
α Perſei	+16		γ Draconis	+12	
η Pleïadum		−16	α Lyræ	inſenſ.	+14
γ Eridani	+14		γ Aquilæ		−20
ε Tauri		−11	γ Capricorni	+19	
α Aurigæ	+11	−11	ε Pegaſi		−28
β Orionis	inſenſ.	inſenſ.	δ Capricorni	+24	−17
β Tauri	−11	−13	α Aquarii	+13	
α Orionis	inſenſ.	−11	ζ Pegaſi		−13
μ Geminorum	−16		Fomahand	+21	
ρ Navis	−13	−11	β Pegaſi	+12	
β Cancri		−14	α Andromedæ		−21
ι Urſæ majoris	−54		β Caſſiopeæ	+34	

to which it belongs; and even of the ſidereal ſyſtem, of which this ſun may poſſibly be a member. We ſee then, that while the ſun, by a proper motion, is going towards a certain point of the heavens, each of the ſtars belonging to the ſidereal ſyſtem, of which the ſun is one, ſuppoſing them to be relatively at reſt, with reſpect to each other, will be affected in the manner I have ſhewn (p. 261, &c. of the Paper on the proper Motion of the Sun) notwithſtanding the whole ſyſtem ſhould have a real motion in abſolute ſpace, and change its ſituation with reſpect to other ſyſtems or interſyſtematical ſtars. We ſee alſo, that with reſpect to ſtars not belonging to our ſyſtem, no parallax can appear but what is compounded of the proper motion of the ſun, and of the whole ſyſtem to which it belongs. And ſhould there ever be found, in any particular part of the heavens, a concurrence of proper motions of quite a different direction, we ſhall then, perhaps, begin to form ſome conjectures beſides thoſe already mentioned by Mr. MICHELL (p. 253, of the ſame volume of Tranſactions) which ſtars may poſſibly belong to ours, and which to other ſyſtems.

TABLE

TABLE II.

Polaris		+13	ζ Hydræ		+24
γ Ceti	−14		α Hydræ		+13
β Perſei	−10		β Herculis	+14	
α Leporis		+11	γ Cygni	−13	
μ Geminorum		+15	ε Pegaſi	−14	
ε Canis majoris		+10	ζ Pegaſi	−20	

From the firſt table we gather, that the principal ſtars, Lucida Lyræ, Capella, α Orionis, Rigel, Fomahand, α Serpentarii, α Aquarii, α Arietis, α Perſei, α Andromedæ, β Tauri, β Ceti, and twenty more of the moſt diſtinguiſhed of the ſecond and third rank of ſtars, agree with our propoſed ſolar motion; when, on the contrary, the ſecond table contains but a few ſtars, and not a ſingle one of the firſt magnitude amongſt them to oppoſe it. It is alſo remarkable, that many ſtars of the firſt table agree both in right aſcenſion and declination with the ſuppoſition of a ſolar motion, whereas there is not one among thoſe of the ſecond table which oppoſes it in both directions. This ſeems to indicate that the ſolar motion, in ſome of them at leaſt, has counter-acted, and thereby deſtroyed the effect of their own proper motion in one direction, ſo as to render it inſenſible; otherwiſe it would appear improbable, that eight ſtars out of twelve, contained in the latter table, ſhould only have a motion at rectangles, or in oppoſition to any one given direction. The ſame may alſo be ſaid of nineteen ſtars among thoſe of the former table, that only agree with the ſolar motion one way, and are as to ſenſe at reſt in the other direction; but theſe ſingularities will not be near ſo remarkable when we have the motion of the ſun to compound with their

own proper motions. However, I forbear entering too much into refined conſideration; what we are chiefly to determine at preſent is, an outline or ſketch of what many repeated, and farther extended, obſervations muſt ripen ſo far as in time to enable us to apply more particular calculations.

The motions of α Lyræ and ε Urſæ majoris towards the north are placed in the firſt table; it will, therefore, be proper to ſhew the general law by which the apparent declinations of the ſtars, at preſent under conſideration, are governed. Let an arch of 90 degrees be applied to a ſphere repreſenting the fixed ſtars, ſo as always to paſs through the apex of the ſolar motion: then, while one end of it is drawn along the equator, the other will deſcribe, on the ſpherical ſurface, a curve which will paſs through the pole of the equator, and return into itſelf at the apex. This curve, to borrow a term from natural hiſtory, is a *non-deſcript* as far as I can find at preſent, and may be called a ſpherical conchoid from the manner of its generation. The law then is, that all the ſtars in the northern hemiſphere, ſituated within the nodated part of the conchoid, will ſeem to go to the north by the motion of the ſolar ſyſtem towards its apex; the reſt will appear to go ſouthwards. A ſimilar curve is to be delineated in the ſouthern hemiſphere, in the nodated part of which the ſame appearances will take place. It will require but little attention to ſee the truth of this conſtruction.

Suppoſe the great circle A*cam*, fig. 5. of which the generating quadrant *mn* is a part, compleated; then will it interſect the equator EQT in two oppoſite points *me*. Now, ſince the apex A, by the hypotheſis, is ſomewhere north of the equator, the great circle will always make ſome angle A*m*Q with it; and the point *n*, which is 90 degrees from the interſection *m*

with the equator, will be the moſt northern part of the ſemicircle *mnc*. From what has been ſaid (p. 261. of the paper on the Motion, &c.) it follows, that the apparent motion of any ſtar *s*S will always be in an arch of a great circle A*s*S*c* drawn through the apex A and ſtar *s*S: therefore, if the ſtar be leſs than 90 degrees of the generating circle diſtant from its interſection with the equator (having more northern declination than the apex) as at *s*, its northern declination will increaſe, and it will alſo fall within the nodated part of the conchoid *Ax*P*y*; but when its diſtance from the interſection *m* is more than 90 degrees, as at S, the motion will be towards the ſouth, and the ſtar will be ſituated without the nodated part of the curve. That the ſtar *s* will fall within the nodated part appears becauſe *ms* being leſs than *mn* by ſuppoſition, if *m* be drawn towards E, to deſcribe the conchoid the angle A*m*Q will decreaſe, and therefore the deſcribing point *n* will be depreſſed below *s* as it approaches A. For the ſame reaſon S will fall without; ſince, by drawing *m* towards Q, the angle A*m*Q will become greater than S*m*Q, and the deſcribing point *n* will paſs above the ſtar S. The application of this theory is very ſimple; for inſtance, let it be required to find whether any given ſtar will fall within or without the conchoid. Then, in fig. 6, there will be given P*s*, the polar diſtance of the ſtar; and QP*c*, the difference of right aſcenſion between the ſtar and the apex of the ſun's motion A; alſo, the polar diſtance PA, and declination *c*A of the point A. Then, by trigonometry, the ſides *s*P, PA, and the included angle being given, we find the ſide A*s* and angle PA*s*. Again, the ſide *c*A, and angle *c*A*m*=PA*s* of the right-angled triangle A*cm* being given, we find the hypothenuſe A*m*; and if A*m*+A*s* be leſs than 90 degrees, the ſtar falls within the conchoid, otherwiſe without.

It

It will be found, that I have placed the want of ſenſible motion of α Lyræ and α Orionis in right aſcenſion, and of Rigel both in right aſcenſion and declination to the account of thoſe ſtars that are in favour. Theſe ſtars are ſo bright, that we may reaſonably ſuppoſe them to be among thoſe that are neareſt to us; and if they had any conſiderable motion, it would moſt likely have been diſcovered, ſince the variations of Sirius, Arcturus, Procyon, Caſtor, Pollux, &c. have not eſcaped our notice. Now, from the ſame principle of the motion of the ſolar ſyſtem, by which we have accounted for the apparent *motion* of the latter ſtars, we may account for the apparent *reſt* of the former. Thoſe two bright ſtars, α Lyræ and α Orionis, are placed ſo near the direction of the aſſigned ſolar motion, that from the application of my ſecond theorem (p. 261. of the paper on the Motion of the Solar Syſtem) their motion ought to be inſenſible in right aſcenſion, and not very conſiderable in declination, all which we find is confirmed by obſervation. With reſpect to Rigel and α Serpentarii, admitting them both as ſtars large enough to have ſhewn a proper motion, were their ſituation otherwiſe than it is, we find that they alſo ſhould be apparently at reſt in right aſcenſion; and Rigel having ſouthern declination, and being a leſs conſiderable ſtar than α Orionis, which ſhews but 11″ motion towards the ſouth in 50 years, its apparent motion in declination may, on that account, be alſo too ſmall to become viſible.

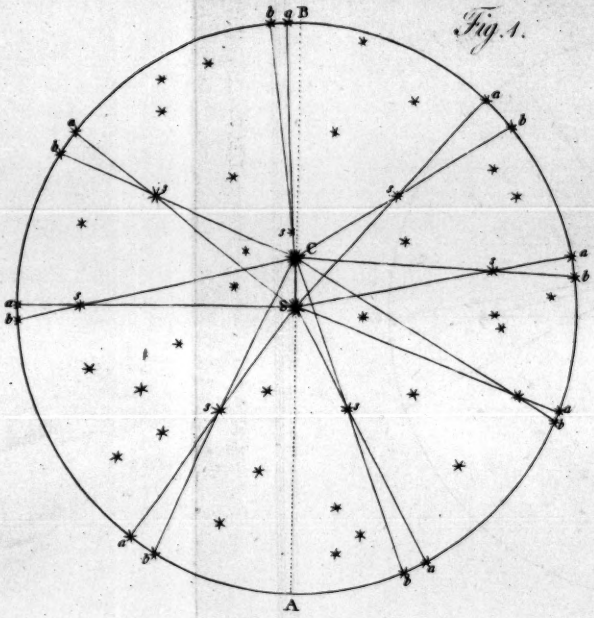
I ſhould not omit to take notice of a very remarkable paragraph of MAYER's, which ſeems to contain a ſtrong objection againſt the motion of the ſolar ſyſtem, while indeed it may be ſhewn to be a very good argument in its favour. At the end of his tract, De Motu Fixarum, he ſays: "Tandem, quum et quæri "poſſit, quæ hujus motus cauſa ſit, hoc unum monere viſum,

" illum explicare non poſſe per motum totius ſyſtematis ſo-
" laris, et ſi nec impoſſibile ſit, ſolem, ut ejuſdem cum fixis
" naturæ, inſtar harum quarundam in ſpatio mundano pro-
" moveri. Nam ſi ſol et cum ipſo planetæ omnes noſtrumque
" domicilium terra, recta tenderent verſus plagam aliquam, uni-
" verſæ fixæ, quæ in ea plaga adparent paullatim a ſe invicem
" diſcedere, et quæ ſunt in oppoſita parte coeli coire viderentur;
" non ſecus ac per ſilvam ambulanti arbores, quæ ante viam
" ſunt, disjungi videntur, quæ a tergo, congredi." Now, if we recollect what has been ſaid of the motion of the ſtars, we find, that thoſe, towards which I ſuppoſe the ſolar ſyſtem to move, do really recede from each other: for inſtance, Arcturus from α Lyræ; α Aquilæ and α Aquarii from α Serpentarii and ε Urſæ majoris; and, on the contrary, thoſe in the oppoſite part of the heavens do really come nearer to each other; as Sirius to Aldebaran; Procyon to α Arietis; Caſtor, Pollux, Regulus, &c. to α Ceti, α Perſei, α Andromedæ, &c. All this agrees with what MAYER ſays ought to happen, if the ſolar ſyſtem was to have a motion towards a certain part of the heavens; which, by the bye, I find this admirable aſtronomer mentions as a very poſſible thing *. However, when he ſays that *all the ſtars* in thoſe parts towards which the ſun might be ſuppoſed to move, ſhould recede from each other, and *vice verſâ*; I muſt add, that this would only take place under the reſtrictions of my firſt, ſecond, and third theorems, and therefore it is not to be expected, that we ſhould immediately ſee the effect of this parallax in any but the ſtars that are neareſt to us. But as we have at preſent no other method of judging of the relative diſtance of the fixed ſtars than from their apparent bright-

* This paper, De Motu Fixarum, was read at Gottingen in January 1760.

neſs,

Fig. 1.



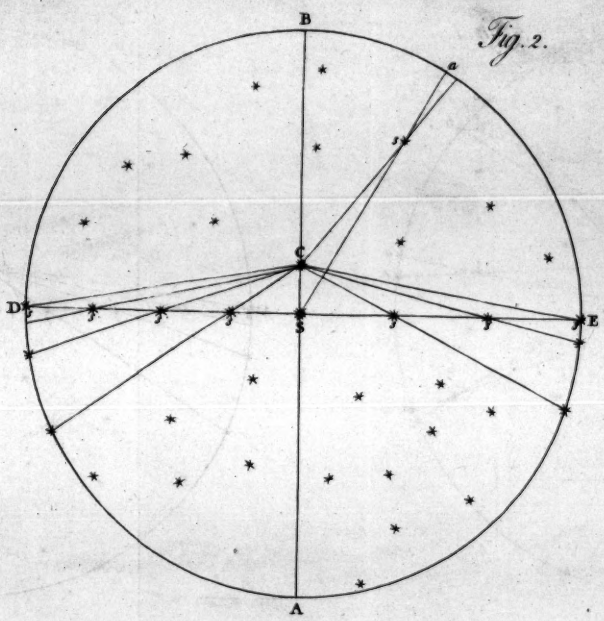
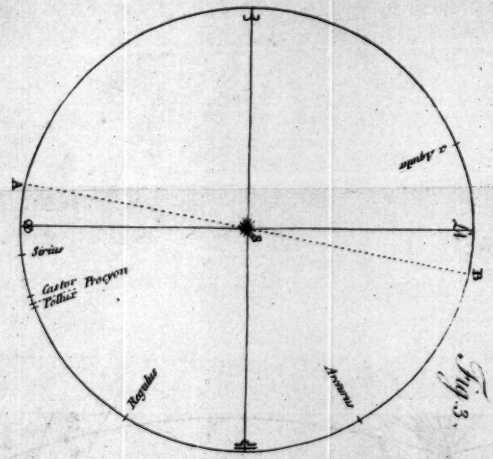
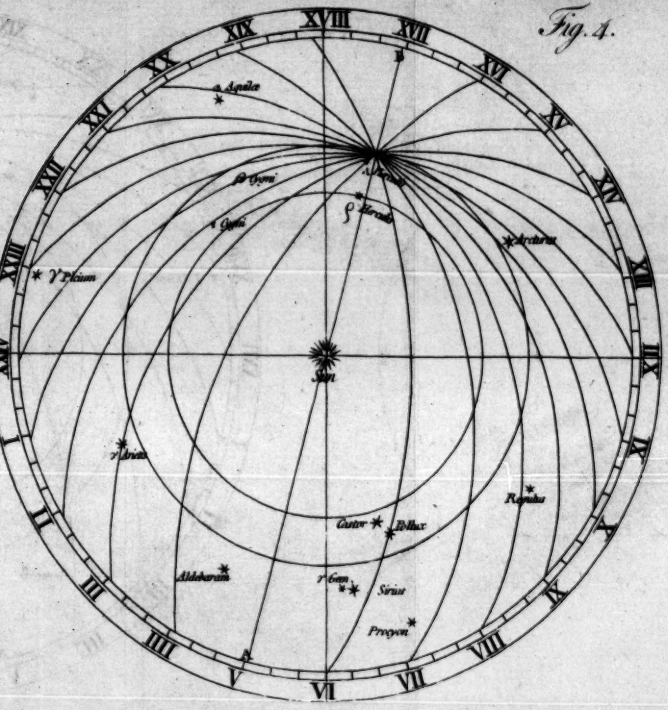


Fig. 2.


Sirius
Castor Procyon
Pollux
Regulus
Arcturus
Fig. 3.

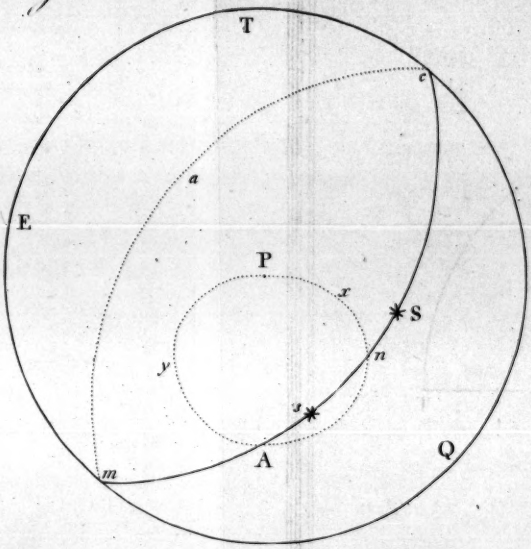
XXIV
XXIII

Fig. 4.



Bayerc. S.

Fig. 5.



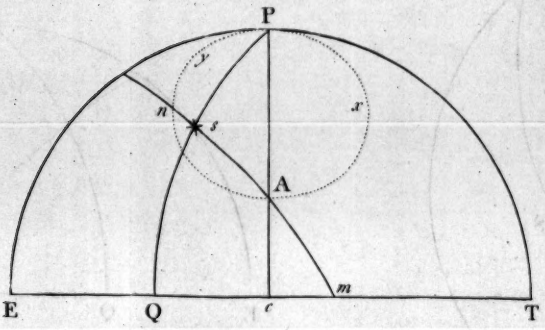


Fig. 6.

neſs, thoſe that are moſt likely, on that account, to be affected by a parallax ariſing from the motion of the ſolar ſyſtem, are the very ſtars which, by MAYER's own table, I have made uſe of to point it out to us *.

Datchet, March 13, 1783.

* I have lately been favoured by Dr. WILSON, Profeſſor of Aſtronomy at Glaſgow, with a ſhort tract, called, "Thoughts on general Gravitation, and Views "thence ariſing as to the ſtate of the Univerſe;" wherein the poſſibility of a Solar Motion is alſo ſhewn. It was printed in 1777. Mr. DE LA LANDE, in the Memoirs for 1776, with his uſual felicity of thought, has inferred the probable motion of the ſyſtem from the ſun having a rotation round his axis, when he ſays, p. 513. "Une force quelconque imprimée à un corps, et capable de le "faire tourner autour de ſon centre, ne peut manquer auſſi de déplacer le centre, "et l'on ne ſauroit concevoir l'un ſans l'autre. Il paroit donc très-vraiſemblable "que le ſoleil a un mouvement réel dans l'eſpace abſolu," &c.

XVIII. *Some Experiments upon the* Ochra friabilis nigro fuſca *of* Da Coſta, *Hiſt. Foſſ. p.* 102.; *and called by the Miners of* Derbyſhire, Black Wadd. *By* Joſiah Wedgwood, *F. R. S.*

Read March 13, 1783.

THE extraordinary circumſtance of this ſubſtance taking fire upon being ſlightly mixed with linſeed oil, firſt diſcovered by accident in the year 1752, at Mr. BASSANO'S, a painter in Derby, has rendered it a ſubject of curioſity; but, as it is now employed in conſiderable quantities, and very advantageouſly, as an oil-colour in ſhip and houſe-painting, it has a better claim to our attention; and it is hoped, an attempt towards the further inveſtigation of ſo curious and uſeful an earth may be acceptable to this illuſtrious Society.

It is many years ſince I firſt collected ſome of this earth, which baſſetted out in a hollow way near Winſter, in Derbyſhire, and tried ſome experiments upon it; but as they were not very intereſting to me at that time, and my hands being full of other matters, I made no further uſe of it till December laſt, when a ſeries of experiments being made at the Preſident's houſe upon its inflammable property when mixed with oil, at which JOHN WALSH, Eſq. and ſeveral other gentlemen were preſent, Mr. WALSH was kind enough to ſend me a ſpecimen from thence, and expreſs his deſire that I would analyſe, and make ſome further experiments upon, this extraordinary ſubſtance.

Mr.

Mr. WOODWARD, as well as Mr. DA COSTA, has deſcribed this earth ſo minutely, that it cannot eaſily be miſtaken; but from the following experiments it will appear, that it ſhould not be claſſed amongſt the *Ochres not acted upon by acids*; and that it may, with as great propriety, be called *Manganeſe* as *Ochre*.

EXP. 1. Mixed with porcelain biſcuit body, it gives darker or lighter ſhades of black and brown, as the quantity is greater or leſs in proportion to the body.

EXP. 2. Mixed with linſeed oil, in the quantity of a few penny-weights only, into a paſte, it dried very ſlowly, without producing any perceptible ſmoke or heat. The quantity, perhaps, was too ſmall for ignition, and, I believe, it was over-doſed with oil.

EXP. 3. When the mineral was previouſly calcined with a ſlight red heat about half an hour, the mixture of it with the oil dried much ſooner and harder; a circumſtance which, if not already known, may render it ſtill more valuable to the painter. In other reſpects no difference could be obſerved.

EXP. 4. In the above low heat it ſuffers no alteration of colour or texture. In a heat of 30°, by my thermometer for meaſuring high degrees of heat, it loſes its property of ſtaining the hands, diminiſhes very conſiderably in bulk, acquires a little hardneſs, though it ſtill proves friable between the fingers, and has its colour changed from a browniſh to a blueiſh black. In a heat of 80° it begins to melt; and at 95° runs into a black ſcoria.

EXP. 5. With black flux, in a heat of 90°, by the above-mentioned thermometer, it yielded a button of lead, amounting, in one experiment, to 21; and in another to 22 grains from an ounce, or nearly $\frac{1}{12}$.

EXP.

EXP. 6. Water extracts nothing from it. The mineral acids, with the aſſiſtance of heat, diſſolve about eleven parts out of twelve; but a large quantity of acid is neceſſary for this ſolution. The reſiduum is greyiſh white, full of bright micaceous particles, with a few fine filaments like thoſe of aſbeſtos, which ſuffer no change in a moderate red heat. In a heat of 144°, which is 14° beyond the fuſion of caſt iron, it ran into a perfect glaſs; but whether this was a vitrification of the pure earth itſelf, or of a combination of it with the argillaceous matter it was in contact with, the ſmallneſs of the quantity did not admit of aſcertaining. Upon the Heſſian crucible it formed a black glaſs; what adhered to the thermometer-piece was brown.

EXP. 7. On boiling with oil of vitriol to dryneſs, the bottom and ſides of the maſs became red like colcothar, the middle white, the intermediate parts yellow or reddiſh yellow, and ſome greeniſh. Theſe appearances were at firſt attributed to a vitriol of iron in different degrees of calcination; but, on ſeparating ſome of the purer white and red parts, the former were found to produce in vitrification the ſame colour as manganeſe does, the latter the ſame as colcothar; the other ſeemed to be a mixture of the two.

EXP. 8. A ſolution of the mineral in nitrous acid was precipitated, inſtead of common alkali, with Pruſſian lixivium, which has the property of throwing down from acids iron, manganeſe, and all metallic bodies, but no one of the earthy claſs. When the addition of this lixivium ceaſed to make any further precipitation, common alkali, added afterwards, had alſo no effect; a proof that this mineral contains no earth ſoluble in acids, for that would have remained in the liquor after the

the precipitation of the other matters by the Pruſſian lixivium, and been precipitated by the alkali added at laſt.

EXP. 9. On precipitating a like ſolution by gradual additions of alkaline lixivium, and ſeparating the precipitates as often as a freſh addition of the alkali occaſioned any different appearance from what the preceding had done; the firſt precipitate was white; the next of a ruſty red colour, like precipitate of iron; the laſt very white, while diffuſed through the liquor, and when ſettled, but in drying turned a little brown. The firſt, which was in a very ſmall quantity, as nearly as could be judged by weighing the filters, about a twentieth part of the other two, was found to be lead; the ſecond was iron; and the third manganeſe, nearly in equal quantities, all pure, or very nearly ſo, from one another.

It appears from theſe experiments, that 22 parts of this mineral contain nearly two of indiſſoluble earth, chiefly micaceous, 1 of lead, about 9½ of iron, and the ſame quantity of manganeſe.

Specimens of the colours produced by vitrification.

O. The mineral itſelf.

1. 2. 3. The firſt, ſecond, and third precipitates.

XIX. *Mémoire ſur la Manière de préparer, avec le moins de perte poſſible, le Sel fuſible d'Urine blanc, et pur, et l'Acide phoſphorique parfaitement tranſparent. By the Duke de* Chaulnes, *F. R. S. preſented by Sir* Joſeph Banks, *Bart. P. R. S.*

Read March 13, 1783.

JE déſirais depuis longtems de m'occuper de ces objets, lorſque les circonſtances me mirent, en 1773, à portée d'y employer le tems néceſſaire et de me procurer facilement et en quantité les matériaux qui y ſont indiſpenſables.

Je crus devoir, avant tout, commencer par m'inſtruire des travaux qu'on avait entrepris ſur ce ſujet.

Parmi les anciens auteurs, RAYMOND LULLE, en avait dit quelque choſe, ſur le ton énigmatique et faſtueux des rêveries alchimiques.

On trouve les manuſcrits inconnus d'un vieux Allemand nommé TURNEISSER, cités par M. POTT.

Le grand BOERHAAVE lui-même, avait dit ſur ces ſubſtances quelques mots qui ne ſont dignes de cet homme illuſtre, que relativement au tems où il écrivait.

Deux Allemands plus modernes et très-peu connus dans ce pays, appellés Meſſ. HAUPT et SCHLOSSER, s'en ſont occupés depuis. Je me ſuis procuré la diſſertation du dernier, mais je n'ai pû ſcavoir autre choſe, des travaux de M. HAUPT, que ce qu'en ont rapporté Meſſ. POTT et MARGRAAF.

Ces deux derniers enfin, ont donné sur le sel fusible : le premier, une dissertation assez longue et fort obscure, traduite par M. DE MACHY : et le second un mémoire qui est ce que nous avons de mieux sur cette matière, et que l'on trouve dans ses opuscules chimiques, ainsi que deux autres, où il en est dit quelque chose relativement au phosphore, qui en est l'objet principal.

La foule d'observations qu'il rapporte, dénote un travail immense, mais bien confus ; je fus, surtout, étonné de trouver plusieurs faits faux, dans les ouvrages de ce savant, et j'oserais à peine l'avancer, si je n'étais à portée de le démontrer par des observations trop simples pour que je puisse être taxé de les avoir mal faites ; comme il est impossible de suspecter les connoissances de ces artistes célébres, je crois qu'il vaut mieux s'en tenir à regretter qu'ils ayent suivi la méthode, qui étoit ordinaire aux chimistes Allemands de ce tems, trop infectée d'idées alchimiques, et qui est non seulement très-embrouillée en général, mais qu'ils cherchent encore à rendre obscure, comme le dit très-bien la lettre suivante. Leur but paroit avoir été beaucoup plus de prouver qu'ils avaient des connoissances que de tendre à les communiquer. On trouve quelque clarté dans les faits, mais nuls détails dans la manière d'obtenir les resultats ; il faut soi-même travailler, avec peine, pour y parvenir. Il est vrai qu'on les trouve alors constans pour la plus part.

J'étais déjà venù à bout de me procurer du sel fusible et de l'acide phosphorique, lorsque je reçus la lettre qui suit de M. ROUELLE, célébre chimiste de Paris.

" Vous avez raison, Monsieur le Duc, de dire que M. MARGRAAFF est minutieux et qu'il ne s'explique guères, sur les " moyens de préparer le sel fusible ; je crois qu'il en a fait un " mystère, car il dit que son sel fusible, lorsqu'il est bien pré-

" paré, donne par la distillation à la cornue de l'alkali volatil, " et que l'acide phosphorique reste dans la cornue, sous la " forme d'une matière transparente comme un verre ; de tous " les chimistes qui ont parlé du sel fusible dans leurs écrits, je " n'en sache aucun qui l'ait préparé tel que M. MARGRAAFF le " décrit ; si vous l'obtenez, vous Monsieur le Duc, conforme à " sa description, je vous prie de m'en envoyer une ou deux " onces "

Je répondis à ce chimiste que j'avais obtenu le sel fusible composé d'acide phosphorique et d'alkali volatil ; que je n'en connoissais point d'autres, que s'il n'avait pas obtenu l'acide phosphorique transparent, c'était faute d'avoir exactement suivi la méthode indiquée par M. MARGRAAFF, dans ses opuscules, c'est-à-dire de lui faire subir dans un creuzet un feu violent, après l'avoir tiré de la cornue.

J'envoyai en même tems à M. ROUELLE, les echantillons qu'il me demandait, et j'ai trouvé dans la suite qu'il en avait préparé, d'à-peu-près pareil à celui que je lui avais envoyé, mais que j'ai reconnu depuis pour être le sel fusible de première crystallisation, qui est très altéré par le sel marin, dont je vais parler, et dont le caractère principal est de ne pouvoir fournir le phosphore. Je fus toute-fois bien aise de voir qu'on ne s'était point occupé de cette matière.

Pour achever de connaître les travaux qu'on avait pu entreprendre à ce sujet, je consultai Mess. MACQUER et BAUMÈ dès que je fus de retour à Paris. Ils me confirmerent dans l'opinion qu'il n'y avait point de procédés clairs et précis, pour obtenir le sel fusible ; M. BAUMÈ me dit seulement qu'il s'en était procuré en faisant crystalliser de l'urine épaissie ; mais que lorsqu'il avait voulu procéder à une seconde crystallisation, tout avait semblé disparaître *comme par enchantement* ; je savois bien que c'était

un des principaux embarras de la preparation du sel fusible ; mon but est donc aujourd'hui, de donner avec le plus de clarté possible, les moyens de le préparer et de le conserver presque-entiérement, en lui faisant subir les purifications nécessaires. L'évaporation d'une grande quantité d'urine, étant le premier procédé pour avoir du sel fusible, je commençai par consulter les mémoires de M. MARGRAAFF, et ce qu'en dit M. MACQUER très en abrégé dans son Dictionnaire de Chimie (edition de 1776). Le premier veut qu'on le fasse évaporer lentement dans des vaisseaux de terre bien vernissés, jusqu'à la consistance de sirop ; mais quoiqu'en général M. MACQUER ne parle que d'après M. MARGRAAFF, comme il me l'a dit lui-même, il a cependant soin d'observer, d'après les évaporations d'urine, faites ici, qu'il est indifférent de la faire evaporer par une forte ou lente ébullition ; je rapporterai dans la suite du mémoire des faits qui prouvent que le sel fusible ne change de caractère, qu'à un dégré de chaleur beaucoup plus fort.

Si l'on ne pouvait pas évaporer l'urine en la faisant bouillir, il serait bien difficile d'en faire évaporer 20 muids comme je l'ai fait alors.

Le lieu que je choisis pour ce travail était un rempart écarté où j'avais fait placer six vieilles futailles couvertes, dans lesquelles un soldat invalide venait vuider tous les matins des baquets d'urine fournie par une garnison de deux bataillons ; j'avais fait construire auprès de ces futailles un fourneau pareil à celui que décrit M. HELLOT, dans les Mémoires de l'Academie, pour l'année 1737. Le même soldat qui apportait l'urine dans les futailles conduisait l'évaporation pendant la journée, et m'en apportait le produit le soir à mon laboratoire ; le chaudron qui servait à évaporer tenait environ cinquantes pintes, et on le remplissoit trois fois par jour ; quand il ne restoit plus de la totalité,

lité, que trois ou quatre pintes d'extrait, on le ramassoit avec une cuillere de fer, dont le manche étoit perpendiculaire au cuilleron et dont la forme étoit exactement la même que celle du fond du chaudron. Comme l'urine gonfle prodigieusement, toutes les fois qu'on la chaufe de nouveau, ou lorsqu'on méle de la nouvelle liqueur avec celle qui est déjà chaude, je trouvais plus simple, et je craignois moins le gonflement, en n'ajoutant qu'environ une pinte à chaque fois.

Lorsque j'évaporai l'urine de cette manière, je ne connoissais pas encore l'usage que M. BAUMÉ fait des galeres; cette méthode est sans contredit la meilleure, je viens de m'en servir tout nouvellement, pour faire évaporer trois muids de cette liqueur, dans deux chaudrons de 150 pintes chacun, scellés sur un fourneau de cette espéce, construit avec de mauvais platras, en ne mettant dans chacun de ces deux chaudrons qu'une cinquantaine de pintes d'urine à la fois, et ne les enfoncant dans le fourneau que jusqu'au tiers de leur hauteur; de cette maniere, le gonflement n'est point à craindre, l'on est dispensé des attentions, et l'on consomme beaucoup moins de bois, pour évaporer la meme quantité d'urine.

Le commencement de l'évaporation (comme je l'ai déjà dit) n'est point difficile à conduire, et ne demande presque point d'autre soin que de veiller, au gonflement, soin dont l'on est même dispensé par le procédé qui précède, mais la fin en demande beaucoup, et qui sont même indispensables alors, si l'on veut obtenir le sel fusible presque pur dés sa premiere crystallisation.

L'urine est composée de parties salines extractives, et savoneuses, les dernieres ne sont presque d'aucune considération dans la préparation du sel fusible, mais les premieres le sont si fort, que presque tout l'embarras du procédé ne vient que de la grande

quantité

quantité de sel marin contenu dans l'urine, qui cryſtalliſe très facilement, et pèle et mêle avec le sel fusible.

Le moyen que j'ai employé pour les ſeparer, et qui ſe préſente naturellement à ceux qui connoiſſent la nature de ces ſels, eſt celui de cryſtalliſer le ſel marin, par l'èvaporation, et le ſel fuſible, par le refroidiſſement, mais ce moyen eſt ſujet à pluſieurs difficultés dans la pratique.

Non ſeulement la liqueur qui eſt épaiſſe et ſiropeuſe filtre mal, mais les filtres ſeroient bientot bouchés et la cryſtalliſation qui ſe ferait obſtruerait tout, ſi l'on n'obſervoit diverſes précautions pour s'en garantir.

Celles que j'employe, ſont d'interrompre l'évaporation dès qu'il commence à ſe précipiter du ſel marin au fond de la liqueur, et de la faire paſſer alors par un tamis clair, en l'agitant toujours, pendant qu'elle eſt encore bien chaude. Ce procédé ſépare tout le ſel marin, qui s'eſt cryſtalliſé par l'évaporation, et en débarraſſe le ſel fuſible qui ſe cryſtalliſe enſuite par le réfroidiſſement.

Une ſeconde filtration eſt cependant néceſſaire pour mieux dégager la liqueur des petites portions de ſel marin, qui peuvent avoir paſſé par les interſtices du tamis.

La maniere dont je ſépare ces petites portions de ſel marin, eſt de filtrer la liqueur en la conſervant la plus chaude qu'il eſt poſſible, et voici, quel eſt le procédé que j'employe pour cet objet: je ſoutiens un tonneau à environ un pied de diſtance de terre, à l'aide de quatre pieds de bois qui y ſont ajuſtés; le fond inférieur eſt percé d'un trou de ſix pouces de diamètre, par lequel je fais paſſer la pointe d'une chauſſe de toile, ſoutenue par en haut au moyen de quatre bâtons croiſés qui repoſent ſur les bords du tonneau, et le fond inférieur eſt doublé, en dedans, de tôle, pour pouvoir mettre de la braiſe deſſus; enfin l'on

paſſe

passe une terrine sous la pointe de la chausse, et l'on glisse sur le dessus du tonneau un couvercle qui, par ce moyen, le ferme au degré que l'on veut.

Il est facile de voir, d'après la description de cet appareil, qu'on est le maitre du degré de châleur qui règne dans l'intérieur du tonneau pendant la filtration, et qu'on prévient de cette manière la cryſtalliſation qui embaraſſeroit le filtre; la liqueur qui paſſe dans la terrine, qui eſt froide, peut cryſtalliſer ſans nuire à l'opération, et on la porte alors à la cave, où on la laiſſe achever de cryſtalliſer à volonté.

La principale et la vraie obſervation qui quoique ſimple, n'a été faite par aucun des auteurs qui ont parlé de cette cryſtalliſation, eſt que ſi l'on a employé l'urine fraiche, le réſidu eſt beaucoup plus épais et plus ſiropeux, que ſi l'on a commencé à laiſſer putréfier l'urine, quelques ſemaines avant l'évaporation, les réſidus ſont alors beaucoup plus clairs, ſe filtrent mieux, la ſéparation des ſels eſt plus exacte, et ils cryſtalliſent ſi aiſément qu'ils le ſont quelque ſoin entierement au bout de 24 heures, comme l'a fort bien remarqué le ſeul M. SCHLOSSER pendant que BOERHAAVE vouloit qu'on laiſſat évaporer pendant une année entiére le réſidu de l'urine évaporée.

Ces auteurs, au reſte, ont bien rapporté leurs ſentimens divers, mais n'ont point connu le principe d'ou procédoit la différence de leur avis: M. BAUMÉ eſt le prémier, que je ſache, qui ait imprimé, qu'on pouvait conduire une cryſtalliſation à ſon gré en épaiſſiſſant plus ou moins la liqueur, avec une liqueur épaiſſe qui n'eut aucune action ſur le ſel qui y étoit contenu.

Quand la liqueur étant très froide, tout le ſel eſt cryſtalliſé contre les parois de la terrine, il faut prendre les précautions ſuivantes pour le retirer. On commence par verſer à part la premiere moitié qui eſt la portion la plus claire, enſuite après avoir

avoir détaché avec une spatule tout le sel adhérent à la terrine, on le verse avec la seconde sur un tamis de crin ordinaire, la liqueur entraine par ce moyen toutes les saletés fines qui peuvent y être suspendues, et le sel reste sur le tamis.

On passe une seconde fois dessus de la meme manière la prémiere portion de la liqueur qui est la plus claire, qui se trouvant saturée ne dissoût aucune portion de sel et la nettoye encore d'avantage.

Pendant que le sel fusible est encore humide de ces deux lotions, et après l'avoir ressuyé sur du papier gris, je procède à une troisieme pour en lever la partie extractive par laquelle il est encore sali.

J'employois d'abord pour cet effet l'eau commune bien saturée à chaud de sel marin, et filtrée après son refroidissement, mais j'ai quitté cet usage parceque j'ai reconnu que cette eau, quoique bien saturée de sel marin, pouvoit encore dissoudre une bonne quantité de sel fusible; cette raison m'a déterminé à substituer dans le mortier où je mets mon sel fusible encore humide, de l'esprit de vin très rectifié à la dissolution saline dont je viens de parler: après avoir bien remué avec une spatule, cet esprit de vin avec le sel qui y est contenu, je verse le tout sur un tamis, comme auparavant; l'esprit de vin passe et suffit quoiqu'en très petite quantité pour enlever presque toute la matière colorante par laquelle le sel est terni, il est nécessaire pour que cette opération réussisse bien, d'avoir d'abord égrugé le sel fort menu, parce que c'est entre les lames des cristaux que sont contenues les saletés.

Le sel ayant été égouté sur le papier gris est un peu plus blanc que le sel commun gris dans son état ordinaire, et peut être rendu alors parfaitement blanc par une seconde cristallisation, mais qui est sujette à bien des difficultés dont les auteurs

n'ont point fait mention, et sur lesquelles meme ils donnent le change.

M. MARGRAAFF conseille pour procéder à cette seconde cristallisation du sel fusible, de le dissoudre tout simplement dans l'eau chaude, de le laisser cristalliser, d'évaporer ensuite ce qui reste de liqueur, on retire encore un peu de sel, dit-il par la cristallisation; enfin il ajoute qu'en répétant 3 ou 4 fois ce procédé, on obtient le sel fusible parfaitement blanc et pur.

Je ne sais comment M. MARGRAAFF l'entend, mais ce qu'il y a de certain, c'est que si on répétoit trois ou quatre fois ce procédé, on ne retireroit pas un atôme de sel fusible; si l'on en fait dissoudre à la fois seulement une livre et demie ou deux, on en retire avec peine le tiers de la premiere quantité, et quand une fois on a fait évaporer le restant de la dissolution, à peine en retire-t-on quelques atomes, c'est ce qui est arrivé à M. BAUME' lorsqu' ayant obtenu par une premiere cristallization 8 ou 10 livres de sel fusible, à peine a t'il pû en conserver quelques onces par une seconde.

Je n'ai pas trouvé ce phénomène tout à fait aussi outré qu'on l'assure, mais il est très certain qu'on retire à grande peine le tiers du sel dissous et même beaucoup moins, si on veut le purifier à grandes doses; la raison de cette deperdition est pourtant fort extraordinaire, le sel fusible tel que M. MARGRAAFF le décrit, et tel que je l'ai obtenu, est un composé mi-partie d'alkali volatil et d'acide phosphorique, et il seroit assez simple que l'alkali volatil ne conservat que la plus foible adhérence avec l'acide phosphorique qui est très fixe; mais comment cet acide phosphorique si fixe au feu est-il si facilement entrainé par l'alkali volatil, qui lui est uni si foiblement et qui est d'une nature si différente?

Je vais rapporter les preuves que j'en ai et les diverſes expériences que j'ai faites à ce ſujet, avant de paſſer aux moyens que j'ai trouvés pour lui procurer preſque ſans perte les derniers dégrés de pureté.

Je crois devoir commencer, pour donner des idées nettes ſur le ſel fuſible, par rapporter la décompoſition que j'en ai faite, qui n'eſt guères que la répétition du procedé qu'indique M. MARGRAAFF pour cet objet.

J'ai pris 4 onces de ſel fuſible purifié par l'eſprit de vin que j'ai diſtille à ſec, dans une petite cornue de verre au bain de ſable, toute la maſſe s'eſt fondue, a bouilloné et il a paſſé dans le récipient une once d'alkali volatil fluor : quand il n'eſt plus ſorti de liqueur, j'ai ôté la cornue et l'ayant caſſée, j'en ai retiré une once de ſcories blanchâtres, que j'ai miſes dans un creuſet auquel j'ai fait eſſuyer le feu le plus violent ; elles ſe ſont fondues en un fluor tranſparent : je les ai coulées alors ſur une plaque de fer poli, où elles ont pris l'apparence de la plus belle topaze, qui eſt devenue en ſe refroidiſſant auſſi blanche et auſſi tranſparente que le plus beau ſtras.

J'ai été curieux d'éprouver quelles ſeroient les différences du ſel fuſible de premiere cryſtalliſation par le même procédé, c'eſt à dire de celui qui contenoit néceſſairement beaucoup plus de parties extractives et de ſel marin, et à quel point étoit vrai ce que diſait M. MARGRAAFF, du procédé employé par M. HAUPT, pour purifier ce ſel par la violence du feu.

J'ai pris en conſéquence une livre de ſel fuſible, tel qu'il ſort de l'urine * et ſimplement lavé, j'en ai retiré par la cornue 8 onces

* J'ai répété une autre fois cette expérience et j'ai obſervé une circonſtance bien ſinguliere, dont je n'ai pû penetrer la cauſe, vers le milieu de la diſtillation, il s'éleva dans toute la cornue de petites parcelles brillantes qui voltigeoient ſans ſe

 fixer

onces d'alkali volatil fluor, et il m'eſt reſté 7 onces et demie de ſcories, un peu plus rouſſatres que les premieres. J'ai mis les ſcories dans un creuſet, et les ayant fondues, je les ai coulées de même ſur la plaque de fer poli; au bout d'un quart d'heure de fuſion, elles étoient tranſparentes dans le creuſet, mais elles ont à peine été figées ſur la plaque de fer, qu'elles ſont devenues blanches et opaques comme de l'email blanc, et ont exhalé une forte odeur d'acide marin. Je voulus voir s'il ſerait poſſible de les en purifier par un feu violent, comme le dit M. HAUPT et je les remis dans un creuſet où je les laiſſai éprouver pendant une heure une bonne fuſion, l'odeur d'acide marin continua toujours à s'exhaler du creuſet, mais lorſque je les coulai au bout de ce tems ſur la plaque de fer poli, elles y reſterent abſolument tranſparentes en ſe refroidiſſant, et conſerverent une auſſi belle apparence que le premier acide phoſphorique; mais il eſt bien éloigné d'avoir le meme dégré de pureté et M. MARGRAAFF à raiſon quand il dit que le ſel préparé par M. HAUPT de cette façon n'eſt point le ſel fuſible tel qu'il le decrit.

Effectivement il y a pluſieurs différences frappantes: le dernier acide phoſphorique du ſel fuſible impur, eſt bien auſſi tranſparent que celui qui ſert de baſe au ſel fuſible purifié lorſ-

fixer et qui avoient l'apparence de particules de verre ſouflé, il s'en ſublimoit une portion à la voute de la cornue, mais il en retombait d'avantage ſur le réſidu, et cette derniere partie commençait à s'y diſſoudre, ce qui me força d'interrompre la diſtillation. Pour recueillir cette ſublimation ſinguliere; je detachai donc avec les barbes d'une plume les legeres parcelles qui reſſembloient à du ſel ſédatif ſublimé, et je les mis à part.

Je n'ai trouvé qu'une obſervation ſemblable à celle ci, dans les obſervations ſur l'urine de vache que M. ROUELLE a données et qu'on trouve dans le Journal de Medecine de Novembre, 1773, il y parle d'un ſel acide volatil qui ſe ſublime en parcelles brillantes comme des fleurs de benjoin, et par conſéquent comme celui que j'ai obtenu.

qu'il est nouvellement fait, mais en le laissant exposé à l'air il s'y humecte promptement et y devient opaque; son dégré de déliquescence et d'opacité, denote la quantité d'acide marin qu'il a retenu et qui change entierement sa composition.

L'acide phosphorique du sel fusible pur ou de seconde cristallisation, immédiatement mêlé avec de la poudre de charbon ou toute autre substance contenant du phlogistique, fournit du phosphore; j'en ai retiré par l'intermede du zinck et l'acide phosphorique de sel fusible de premiere cristallisation ou de celui qui contient du sel marin n'en fournit pas par le même procédé.

L'odeur d'acide marin qui se dégage du creuset, pendant la fusion de ce dernier, nous apprend ce qui se passe durant cette opération. L'acide phosphorique, s'unissant à l'alkali fixe minéral, qui sert de base au sel marin, forme avec lui un nouveau sel fusible, et dégage l'acide marin qui étant devenu libre s'échappe en vertu de sa volatilité.

Le sel qui résulte de la purification de M. HAUPT, est donc très différent du sel fusible ammoniacal, décrit par M. MARGRAAFF, qui est le seul qui donne le phosphore, puis qu'il est réellement un sel fusible à base d'alkali fixe, que l'on obtient, exactement pareil, en unissant l'alkali minéral à l'acide phosphorique purifié.

L'acide phosphorique que je vis parfaitement transparent, à mon retour du Havre, chez le chimiste qui m'avait écrit, et que je retrouvai opaque peu de tems après, doit donc être naturellement du sel fusible obtenu par la simple cristallisation de l'urine, et auquel on n'a point fait éprouver la seconde cristallisation, qui en fait effectivement toute la difficulté.

La derniere expérience sur le sel fusible impur que je viens de rapporter, m'apprenait un fait fort singulier. Le sel fusible de seconde cristallization etoit, ainsi que le premier, composé de particules

particules d'alkali volatil et d'acide phosphorique : on pouvait seulement supposer dans celui de premiere cristallization, une portion de sel marin, de plus, dont on n'avoit encore pu le dépouiller, mais le sel marin étant fixe lui même, comment se faisoit il qu'on ne pût retirer qu'à grande peine moitié du poids du sel fusible de premiere cristallisation, lorsqu'on voulait le purifier, par une simple dissolution dans l'eau chaude, et une seconde cristalisation, et pouvoit-il régénérer un sel fusible ammoniacal, comme on me l'avoit dit, en y ajoutant de l'alkali volatil concret. J'en fis l'expérience, et après avoir retiré tout le sel fusible qui voulut cristalliser, après avoir dissout dans l'eau bouillante, et filtré du sel fusible impur, j'ajoutai l'alkali volatil concret * dans la dissolution qui restait ; j'eus une vive effervescence et je comptais obtenir encore beaucoup de sel, mais à ma grande surprise, je n'en obtins pas beaucoup plus que je n'en aurois eû sans cette ressource ; je résolus alors de m'éclaircir par un procédé certain, du sort qu' éprouvait l'acide phosphorique dans cette seconde cristallisation. Je fis en conséquence dissoudre quatre onces de sel fusible de premiere cristallisation dans l'eau froide qui fut nécessaire pour le tenir en dissolution, et ayant distribuè cette liqueur dans 4 assiettes de verre, que j'eus soin de couvrir de gaze, je laissai cristalliser et secher la totalité, puis l'ayant pésé je ne retrouvai qu'une once 6 gros 22 grains ; d'ou pouvait venir cette étrange diminution, de plus d'une once a la simple châleur d'un cabinet où le thermometre de M. DE REAUMUR n'a pas monté plus haut que dix dégrés au dessus de glace ?

* Ce sel a une singularité, c'est que si l'on évapore la dissolution de son acide phosphorique séparé de nouveau de son alkali volatil cet acide phosphorique du sel régénéré, cristallise, pendant que celui du sel fusible ordinaire reste en gelée.

Ce

Ce fait ne constatoit pas moins la certitude de cette étrange évaporation, dès lors il étoit naturel que la chaleur de l'eau bouillante l'augmentât beaucoup, et que pendant la dissolution et la cryſtallisation, il ſe perdit la plus grande partie du ſel fusible.

Je tentai encore un moyen que j'imaginai qui pourroit me procurer une ſeconde cryſtalliſation ſans perte ; ce fut d'eſſayer de faire la diſſolution et la cryſtalliſation dans des vaiſſeaux clos.

Je mis pour cet effet 4 onces de ſel fuſible impur dans un entonnoir garni de papier gris, ſoutenu par des pailles, et je portai l'entonnoir ſur un bocal, que je mis ſur une plaque de verre, puis je couvris le tout avec un récipient, ouvert par en haut, que je lutai à la même plaque avec de la cire molle, et par le trou duquel je verſai enſuite ſur le ſel fuſible de l'eau bien bouillante, je bouchai, enſuite promptement le trou du récipient, et je me flattais de prevenir par ce moyen toute déperdition mais il n'y eut qu'une très legére diſſolution de la ſubſtance, ce qui devait arriver dans des vaiſſeaux bien clos.

Je m'aviſai enfin d'un autre expédient dont la réuſſite fut ſi parfaite, que je puis par ſon moyen retirer 4 livres de ſel fuſible parfaitement blanc et pur, de cinq livres de ſel fuſible, de premiere cryſtalliſation ; voici le procédé.

Je fais chauffer dans un petit matras, à long col, le ſel fuſible, gris, lavé à l'eſprit de vin, que je veux faire diſſoudre juſqu'à ce que la ſurface commence à fariner, je verſe alors deſſus, la moitié de ſon poids, d'eau diſtillée, bien bouillante, que j'ai fait chauffer dans une fiole à médecine, je la fais bouillir encore un moment dans le matras, avec le ſel fuſible, juſqu'à ce que je m'apperçoive qu'il eſt entierement diſſout ; je le verſe enſuite promptement dans un entonnoir, garni de papier gris, et poſé ſur une fiole à médecine, la diſſolution, n'étant faite qu' avec une quantité d'eau égale à la moitié du poids du

ſel,

ſel, ſe figeroit ſur le champ et ne pourroit ſe filtrer, ſi l'on n'avoit recours à l'expédient ſuivant : j'emploie une fiole à médecine à laquelle j'attache une ficelle pour la ſoutenir au deſſus du feu et entretenir bouillante l'eau bouillante, dont je la remplis, je la pôſe en cet état ſur l'intérieur de l'entonnoir, et il en réſulte deux effets ; la panſe ronde de la fiole s'appliquant exactement contre les parois de l'entonnoir, par le moyen du papier gris empêche l'évaporation, et la chaleur de la maſſe, entretient celle de la liqueur, qui par ce moyen filtre bien, paſſe toute entiere et ne peut boucher le filtre en cryſtalliſant. Quand elle a achevé de paſſer entierement par la filtration dans la bouteille inférieure, qui eſt enterrée dans un bain de ſable échauffé, de 30 à 40 dégrés à peu près, pour prevenir la cryſtalliſation pendant le paſſage de la liqueur, je laiſſe le tout refroidir très lentement, et il arrive, ſeulement, alors, que les quatre cinquièmes environ du ſel fuſible cryſtalliſent. Le peu de liqueur, qui reſte ſur les cryſtaux retient la petite quantité de ſel marin, qui a pu échapper aux precautions que l'on a priſes, dans le tems de la premiere cryſtalliſation.

Ce ſel fuſible peut, par ce procédé, ſoutenir l'epreuve á laquelle je le ſoumets pour connoître ſon dégré de pureté, qui eſt d'en mettre quelques grains dans une verre et de verſer deſſus des gouttes d'huile de vitriol bien blanche et bien concentrée ; s'il ne s'en dégage pas d'odeur d'acide marin, c'eſt une preuve qu'il eſt bien ſéparé de tout ſel de cette nature ; mais il faut bien prendre garde à n'employer que l'acide le plus blanc et des petits bâtons de verre, ou d'émail pour l'agiter, car ſi l'on employait de l'acide un peu phlogiſtiqué, ou des pailles au lieu de verre pour remuer l'acide, quoiqu'il fut pur, ou aurait immanquablement une odeur d'acide ſulfureux, qu'il eſt très aiſé de confondre lorſqu'on fait les expériences en petit, avec celle de l'acide marin.

*XX. *Experiments for aſcertaining the Point of Mercurial Congelation. By Mr.* Thomas Hutchins, *Governor of* Albany Fort, *in* Hudſon's Bay †.

Read April 10, 1783.

THE following experiments, to determine the freezing point of quickſilver, were made by the direction of the Royal Society, at Albany Fort in Hudſon's Bay, ſituated in the latitude of 52° 14′ North and 82° Weſt longitude from Greenwich.

The inſtruments uſed in theſe experiments were ſimply thermometers, except the apparatus F and G, furniſhed by Mr. CAVENDISH, and of theſe a more ſatisfactory idea will be formed from the annexed drawing than could be conveyed by words alone; I have, therefore, only ſpecified a few particulars, ſo that each inſtrument may be diſtinguiſhed from another.

I have compared the inſtruments with each other for ſeveral weeks in the various temperatures, to adjuſt, with the greater preciſion, the relative degrees on the ſcales; which was the more neceſſary as they differed very much.

The five firſt experiments were made exactly according to the directions ſent to me by the Society, in order to obtain the point of congelation. The two ſucceeding ones are alſo made in the manner they directed, to endeavour to aſcertain the greateſt degree of contraction mercury is capable of; then follow two

† This paper having been for ſome time miſlaid, could not be printed in its turn. This accounts for the double paging and ſignatures.

experiments made in a different manner by my own ſuggeſtion; and, laſtly, an account of mercury frozen in the open air without the aid of any artificial cold, which will be found to corroborate the preceding experiments, and determine the exact point of congelation to be at 40° below the cypher. I have been careful to mark down every circumſtance attending the experiments, and have added a few obſervations to each of them, to elucidate any uncommon phænomena that occurred.

If theſe experiments ſhould be agreeable to the Royal Society, the merit muſt be attributed to the excellent inſtructions they tranſmitted to me, which left me nothing to do but to follow them; yet I cannot avoid doing juſtice to the ingenious Dr. BLACK, Profeſſor of Chemiſtry at Edinburgh, who favoured me with ſome remarks on the experiments I made in 1775 to freeze quickſilver, and firſt ſuggeſted this method of aſcertaining the point of congelation, which I had the honour of communicating to the Royal Society by the means of SAMUEL WEGG, eſq. whoſe attention to promote the views of the Society can only be equalled by that liberality of ſentiment and great goodneſs of heart which makes him encourage even the moſt feeble attempt than can in the leaſt tend to the improvement of knowledge.

THOMAS HUTCHINS.

Dr.

Dr. BLACK's *Letter referred to above.*

DEAR SIR, Edinburgh, 5th Oct. 1779.

I HAVE read with great pleaſure the experiments made at Hudſon's Bay, upon the congelation of mercury, and obſerve that the author has ſucceeded perfectly in effecting it; but could not determine with preciſion what degree of cold was neceſſary to produce it. This, however, does not ſurprize me, as I have always thought it evident, from Profeſſor BRAUN's experiments, that this degree of cold cannot be diſcovered conveniently by congealing the mercury of the thermometer itſelf. I ſhall not here give my reaſons for this opinion; they would lengthen out this letter too much; I ſhall only propoſe what appears to me the proper manner of making the experiment, which is as follows: provide a few wide and ſhort tubes of thin glaſs, ſealed at one end and open at the other; the wideneſs of theſe tubes may be from half to three-quarters of an inch, and the length of them about three inches. Put an inch or an inch and an half depth of mercury into one of theſe tubes, and plunging the bulb of the thermometer into the mercury, *ſet the tube with the mercury and the thermometer in it* into a freezing mixture, which ſhould be made for this purpoſe in a common tumbler or water-glaſs; and, N. B. in making a freezing mixture with ſnow and ſpirit of nitre, the quantity of the acid ſhould never be ſo great as to diſſolve the whole of the ſnow, but only enough to reduce it to the conſiſtence of Panada. When the mercury in the wide tube is thus ſet in the freezing mixture, it (the mercury) muſt be ſtirred gently and

frequently with the bulb of the thermometer; and if the cold be ſufficiently ſtrong, it will begin to congeal by coming thick and broaſy like an amalgam. As ſoon as this is obſerved, the thermometer ſhould be examined without lifting it out of the congealing mercury; and I have no doubt, that in every experiment, thus made, with the ſame mercury, the inſtrument will always point to the ſame degree, provided it has been made and graduated with accuracy.

I am, DEAR SIR,

Your faithful humble ſervant,

JOSEPH BLACK.

To Mr. ANDREW GRAHAM, Edinburgh.

Thermometers described,

A. Repreſents a mercurial thermometer, with an air-bulb at the top, graduated 628 degrees below the cypher, and marked at every ſecond degree. Makers NAIRNE and BLOUNT; the ſcale box-wood.

B. Another mercurial thermometer graduated to 526° below the cypher, each line repreſenting 2°, made by NAIRNE and BLOUNT; the ſcale box-wood.

C. Is a fine mercurial thermometer, with an air-bulb at the top graduated 2300° below the cypher, each diviſion containing 5°; the ſcale made of box, by THROUGHTON.

D. A ſmall ſpirit thermometer on a box ſcale, made by THROUGHTON, and divided to every ſingle degree down to 160° below the cypher.

E. Another ſpirit thermometer, by the ſame maker (THROUGHTON) graduated 90° below the cypher; the ſcale box.

F. A ſmall mercurial thermometer, on an ivory ſcale, divided at every 5° between 220° above and 250° below the cypher; made by NAIRNE and BLOUNT.

G. Another mercurial thermometer, every way like the laſt mentioned, except only reaching from 215° above to 250° below the cypher; by NAIRNE and BLOUNT.

H. A ſpirit thermometer, made by NAIRNE and BLOUNT, with which I have made meteorological obſervations from the year 1774.

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	C.	D.	E.	F.	G	H	
1781										
Nov. 23	10 AM	+ 2½	+ 5	+ 7	+ 9	+ 8	+ 7	+ 7½		
24	10 AM	− 7	− 4	− 1	+ 2½	+ 2	0	+ 1		
	Noon	+ 2	+ 4	+ 7	+ 9	+ 8½	+ 8	+ 8½		
25	10 AM.	− 4	− 1	+ 1½	+ 4	+ 3½	+ 2	+ 2		
	3 PM.	+11	+13¾	+15	+16½	+15	+15	+15½		
26	10 AM.	+ 4½	+ 7	+ 8	+10	+ 9	+ 7½	+ 8		
27	10 AM.	0	+ 2½	+ 5½	+ 7	+ 6½	+ 5	+ 5½		
	5 PM.	+ 4½	+ 7	+ 9	+11	+10	+ 9½	+10		
28	10 AM	− 9½	− 6	− 2½	+ 2	+ 1½	+ 2	+ 2½		
29	10 AM.	− 3	+ ½	+ 4½	+ 6½	+ 5½	+ 3	+ 3		
30	10 PM	+11	+14	+15½	+16	+15½	+15	+15½		
Dec. 1	10 AM.	+17½	+20	+22½	+21½	+21	+21	+22		
2	11 AM.	+23	+25½	+27	+26½	+26	+27	+27		
3	10 AM.	+27½	+29½	+30½	+30	+29	+30	+30		
4	10 AM.	+18½	+20½	+23	+22	+21	+21½	+21		
5	10 AM.	+25	+26½	+27	+27	+26	+26½	+27		
6	10 AM.	+16	+18	+21½	+21	+20	+20	+19		
11	9 AM.	− 5	− 2½	0	+ 3	+ 2½	− 0½	0		
15	8 AM.	−24½	−22	−20	−13½	−14	−20	−19½		
	Noon	−23½	−20½	−18½	−12	−13	−20	−20		
16	8 AM.	−35	−34	−31	−22½	−23	−31½	−31		
1782										
Jan. 7	8 AM.	−39½	−36½	−35	−25	−25	−31½	−34		
11	4 PM.	−32	−30	−31	−18	−20	−26	−26	−32	
	8 PM.	−34	−32	−32	−23	−24	−32	−32	−34	
12	8 AM.	−44	−42	−40	−29	−29½	−40	−39	−42	
	Noon	−36	−34	−32	−21½	−22½	−33	−32	−37	
	4 PM.	−28½	−26	−25	−16½	−17	−25	−25	−30	
	8 PM.	−14	−12	−10	− 5	− 5½	−11	−10	−16	
13	8 PM.	−19	−18	−15	− 8	− 9	−15	−15	−20	
14	8 AM.	−24	−21	−20	−13	−13½	−20	−20	−24	
	Noon	−22½	−20½	−18	−11½	−12	−19	−19	−23	
	4 P.M.	−24	−22	−20	−13	−14	−21	−21	−25	
	8 P.M.	−30	−28	−26	−17	−18	−23	−23	−30	
15	8 AM	−38	−36	−35	−24	−24½	−36	−36	−37	
	Noon	−32	−30	−27	−18½	−19½	−26	−26	−33	
	4 P.M.	−25	−23	−21	−14	−15	−24	−23	−27	

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	C.	D.	E.	F.	G.	H.	
1782										
Jan. 16	8 AM.	−17	−16	−12	− 7	− 8	−14½	−15	−20	
	Noon	− 7	− 5	− 4	+ 1	− 0	− 4	− 4	−11	
	4 PM.	− 6	− 6	− 5	− 0	− 1	− 6	− 6	−11	
17	8 AM.	−14	−12	−11	− 4	− 5	− 9	− 9	−16	
	Noon	− 6	− 4	− 2	+ 2	+ 1½	− 3	− 3	−10	
	4 PM.	+ 2	+ 4	+ 6	+ 8½	+ 7½	+ 5	+ 5	− 2	
	8 PM	− 4	+ 2	− 1	+ 4	+ 3	− 1	− 1	− 5	
18	8 AM.	− 9	− 8½	− 5	− 0	− 1	− 6	− 6	−11	
	Noon	− 2	− 0	+ 2	+ 4	+ 4	− 0	− 0	− 6	
	4 PM.	− 8	− 7	− 5	− 1	− 2	− 6	− 6	−10	
	8 PM.	−15	−13	−11	− 5	− 6	−11	−11	−16	
19	8 AM.	−12	−10	− 7½	− 4	− 4	− 9	− 9	−14	
	Noon	− 8	− 6	− 4	− 0	− 1	− 5	− 5	−12	
	4 PM.	−11	−10	− 7	− 3	− 4	− 8	− 8	−12	
	8 PM	−20	−18	−15	− 9	− 8	−11	−11	−20	
20	8 AM.	−12	−10	− 7	− 3	− 3	− 8½	− 8½	−13	
	Noon	−12	−10	− 7	− 3	− 4	− 8	− 8	−14	
	4 PM.	−16	−14	−12	− 7	− 6	−13	−13	−18	
	8 PM.	−21	−20	−17	−10½	−11	−17	−17	−22	
21	8 AM.	−38	−36	−33	−24	−25	−38	−38	−37	
	Noon	−28	−26	−23	−16	−17	−25	−25	−30	
	4 PM.	−22	−20	−17	−12	−13	−18	−18	−24	
	8 PM.	−28	−26	−24	−16	−16½	−24	−24	−28	Therm. C broke this day.
22	Noon	−19	−17		− 8½	− 9	−15	−15	−21	
	4 PM.	−16	−14		− 7	− 8	−13	−13	−18	
	7 PM.	−20	−18		− 9	−10	−17	−17	−22	
23	8 AM.	−34	−32		−21	−21½	−30	−30	−34	
	Noon	−16	−14		− 6½	− 7	−13	−13	−19	
	4 PM.	−12	−10		− 4	− 5	− 9	− 9	−14	
24	8 AM.	−14	−12		− 4½	− 5	−10½	−10	−16	
	Noon	− 4	− 2		+ 3½	+ 3	− 1	− 1	− 7	
	4 PM.	+ 2	− 0		+ 5½	+ 4	+ 2½	+ 2	+ 4	
	8 PM.	−12	−10		− 3½	− 4	− 8	− 8	−14	
25	8 AM.	−30	−28		−18	−18½	−25½	−25	−30	
	Noon	−30	−28		−17½	−18½	−25	−25	−30	

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.	
1782									
Jan. 26	8 AM.	−103	−80	−33½	−33	−42½	−42	−46	Quickfilver froze in air.
	9 AM.	−323	−444	−29	−29½	−40	−40	−44	
	Noon	−34	−32	−21	−21½	−30	−29½	−34	
	4 PM.	−30	−28	−17	−18	−25	−25½	−32	
	8 PM	−38	−36	−24	−24	−35	−34	−36	
27	8 AM.	−44	−42	−29½	−30	−40	−40	−43	Quickfil. not frozen.
	Noon	−28	−26	−16	−17	−24	−24	−28	
	4 PM.	−26	−24	−14	−14½	−21	−21	−26	
	8 PM.	−30	−28	−17	−18	−25	−25	−29	
28	8 AM.	−30	−28	−17½	−18	−25	−25	−30	
	Noon	−20	−18	−10½	−11	−17	−17	−22	
	4 PM.	−22	−20	−12	−13	−18½	−19	−23	
	8 PM.	−26	−24	−14	−14½	−22½	−23	−27	
29	8 AM.	−38	−36	−24	−24½	−34	−34	−37	
	Noon	−32	−30	−19	−20	−27	−27	−32	
	4 PM.	−30	−28	−17	−18	−25	−25	−30	
30	Noon	−24	−22	−13	−14	−21	−21	−26	
	4 PM.	−26	−24	−14	−15	−20	−20	−26	
	8 PM	−28	−26	−16	−16	−24	−23½	−28	
31	8 AM.	−34	−32	−20	−21	−28	−28	−33	
	Noon	−24	−22	−13	−14	−20	−20	−26	
Feb. 1	8 AM	−38	−36	−24	−24½	−34½	−35	−38	
	Noon	−28	−26	−16	−17	−25	−25	− 9	
	4 PM.	−22	−20	−12	−13	−19	−19½	−24	
2	8 AM.	−30	−26	−17	−18	−24½	−25	−29	
	Noon	−20	−18	− 9	−10	−15	−15½	−21	
	4 PM.	−21	−19	−10	−10½	−16	−16½	−22	
	8 PM.	−22	−20	−10	−10½	−16	−16½	−22	
3	8 AM.	−18	−16	− 9	−10	−15	−15	−20	
	1 PM.	− 4	− 2	+ 4	+ 3	− 1	− 1	− 8	
	8 PM.	−13	−11	− 4	− 5	− 8	− 8	−14	
4	8 AM.	−12	−10	− 3	− 4	− 7	− 7	−12	
	Noon	− 8	− 6	− 0	− 0	− 4	− 4	−10	
5	8 AM.	−32	−30	−19	−20	−26½	−27	−32	
	Noon	−16	−14	− 6	− 7	−13	−13	−18	
	4 PM.	−12	−10	− 3	− 4	− 7	− 7½	−14	
	8 PM.	−12	−10	− 2	− 3	− 7	− 7	−14	

Thermometers

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.
1782								
Feb. 6	8 AM.	− 4	− 2	+ 3	+ 2	− 1	− 1	− 6
7	8 AM.	−34	−32	−20	−21	−28	−28	−32
	4 PM.	−14	−12	− 5	− 6	−10	−10½	−16
	8 PM.	−16	−14	− 6	− 7	−11	−11	−17
8	8 AM.	−10	− 8	− 2	− 3	− 6	− 6½	−12
	Noon	−10	− 8	− 1	− 2	− 5½	− 6	−12
	8 PM.	−20	−18	− 9	−10	−14½	−15	−20
9	7 AM.	−24	−22	−14½	−15	−20	−20½	−26
	Noon	−22	−20	−11	−12	−17½	−18	−23
	8 P.M.	−29	−26	−16	−17	−24	−25	−28
10	8 AM.	−14	−12	− 3½	− 4	− 9	− 9½	−15
	Noon	− 0	+ 2	+ 6	+ 5	+ 3	+ 2½	− 4
	4 PM.	+ 2	+ 4	+ 7	+ 7	+ 5	+ 5	+ 2
	8 AM.	− 2	− 0	− 6	− 5½	− 3	− 3	− 4
11	8 AM.	−24	−22	−12	−13	−19	−19½	−24
	Noon	−18	−16	− 8	− 9	−14	−14½	−19
	4 PM.	−14	−12	− 5	− 5	−10	−10	−16
	8 PM.	−24	−22	−12	−13	−19	−19½	−24
12	8 AM.	− 2	− 0	+ 5	+ 4	+ 1	− 0	− 5
	Noon	+ 8	+10	+14	+13	+11	+10½	+ 4
	4 PM.	−10	−12	−16	−15	−14	−14	− 7
	8 PM.	− 4	− 2	−10	−11	− 7	− 7	− 0
13	8 AM.	−15	−12	− 4	− 5	−10	−10	−15
	Noon	−12	−10	− 1	− 2	− 5	− 5	−13
	4 PM.	− 6	− 4	+ 1	− 0	− 4	− 4	− 9
	8 PM.	−12	− 9	− 3	− 2½	− 7	− 6	−12
14	8 AM.	− 2	− 0	+ 6	+ 5	− 4	− 3	− 5
	4 PM.	− 2	− 1	− 6	− 3	− 4	− 3	− 5
15	Noon	−10	− 8	− 1	− 2	− 5	− 5½	−12
	4 PM.	− 8	− 6	− 2	− 5	− 5	−11	− 4
16	Noon	− 0	+ 2	+ 7	+ 6	+ 4½	+ 4½	− 3
	4 PM.	+ 4	+ 6	+10½	+ 9½	+ 8	+ 8½	+ 2
	8 PM.	+ 6	+ 4	+13	+12	+ 7	+ 7	− 4
17	8 AM.	+12	−14	−17	−16	−15	−15	− 8
	1 PM.	− 6	− 4	−11	−10	− 9½	− 9	− 3
	4 PM.	− 4	− 2	+ 4	+ 3	0	− 0	− 6
	8 PM.	− 8	− 6	− 0	− 0	− 5	− 5	− 9

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.
1782								
Feb. 18	8 AM.	−12	−10	− 2	− 3	− 5½	− 6	−12
	Noon	−10	− 8	− 1	− 2	− 5	− 5½	−12
	8 PM.	−22	−20	−11	−11	−16½	−17	−22
19	8 AM.	−34	−32	−21	−21½	−29	−29	−33
	Noon	−18	−16	− 7	− 8	−13	−13½	−19
	4 PM.	−12	−10	− 3	− 4	− 8	− 8	−14
	8 PM.	−19	−17	− 8	− 8½	−14	−14	−20
20	8 AM.	−18	−16	− 7½	− 8	−13	−13½	−19
	Noon	− 0	+ 2	+ 7	+ 6	+ 4	+ 3½	− 4
	4 PM.	− 6	− 4	+ 2	+ 1	− 1	− ½	− 7
	8 PM.	− 8	− 6	− 0	− 1	− 4	− 4	−10
21	8 AM.	−30	−28	−17	−18	−25	−25	−30
	Noon	−29	−27	−16½	−17	−24	−24½	−29
	4 PM.	−28	−26	−17	−17	−23½	−24	−28
	8 PM	−34	−32	−20	−21	−27	−27½	−32
22	7 AM.	−82	−06	−34	−34	−42	−42	−46
	1 PM.	−35	−32	−21	−22	−30	−30	−34
	4 PM.	−34	−32	−21	−22	−30	−30	−34
	8 PM.	−39	−38	−24½	−25½	−34	−34	−36
23	7 AM.	−44	−42	−28	−29	−40	−40	−42
	4 PM.	−26	−24	−15	−16	−21	−21	−27
	8 PM.	−35	−33	−21	−22	−30	−30	−34
24	8 AM.	−42	−38	−26	−27	−36	−36	−40
	1 PM.	−18	−16	− 7	− 8	−15	−15	−22
	4 PM.	−12	−10	− 3	− 4	− 8½	− 8	−14
	8 PM.	−14	−12	− 3½	− 4	−10	−10	−15
25	8 AM.	−14	−12	− 4	− 5	−10	−10	−16
	Noon	− 4	− 2	+ 3½	+ 4½	− 0	− 0	− 7
	4 PM.	− 2	− 0	+ 5	+ 4,	+ 1	+ 1	− 4
	8 PM.	− 6	− 4	+ 3	+ 2	− 1	− 1	− 7
26	7 AM.	−22	−20	−11	−12	−18	−18	−22
	4 PM.	−10	− 8	− 1	− 2	− 6	− 6	−12
	8 PM.	−18	−16	− 7½	− 8	−14	−13	−17
27	8 AM.	−24	−22	−13	−14	−20	−20	−25
	Noon	−12	−10	− 4	− 5	−10	−10	−17
	4 PM.	−14	−12	− 4	− 5	−10	−10	−15
	8 PM.	−20	−18	− 9½	−10½	−15	−15½	−20

Thermometers

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.
1782								
Feb. 28	8 AM.	− 20	− 18	− 9	− 10	− 15	− 15½	− 22
	Noon	− 8	− 6	+ 1	− 0	− 4	− 5	− 11
	8 PM.	− 12	− 10	− 6	− 7	− 10	− 10	− 12
Mar. 1	8 AM.	− 6	− 4	+ 3	+ 2	− 2	− 2	− 8
	Noon	− 6	− 4	+ 3	− 2	− 2	− 2	− 7½
	4 PM.	− 14	− 12	− 4	− 5	− 10	− 10	− 14
	8 PM.	− 18	− 16	− 8	− 9	− 14	− 14	− 18
2	9 AM.	− 20	− 23	− 13½	− 14	− 20	− 21	− 26
	4 PM.	− 15	− 12	− 5	− 6	− 10	− 10½	− 16
	8 PM.	− 22	− 20	− 11	− 12	− 17	− 17	− 22
3	8 AM.	− 36	− 34	− 22	− 23	− 31	− 31½	− 35
	1 PM.	− 12	− 10	− 2	− 3	− 7	− 7	− 16
	4 PM.	− 10	− 8	− 2	− 3	− 6	− 6	− 12
	8 PM.	− 18	− 16	− 7	− 8	− 13	− 13½	− 18
4	8 AM.	− 24	− 22	− 12	− 13	− 19½	− 20	− 25
	Noon	− 8	− 6	− 0	− 1	− 4½	− 5	+ 7
	4 PM.	− 12	− 10	− 3	− 4	− 7½	− 8	− 13
	8 PM.	− 20	− 18	− 9	− 10	− 15	− 15	− 18
5	8 AM.	− 32	− 30	− 19	− 20	− 26	− 26	− 30
	Noon	− 16	− 14	− 6	− 7	− 12½	− 13	− 19
	4 PM.	− 14	− 12	− 4	− 5	− 10	− 10	− 16
	8 PM.	− 18	− 16	− 8	− 9	− 14	− 14	− 19
6	8 AM.	− 19	− 17	− 8½	− 9½	− 15	− 15	− 20
	Noon	− 10	− 8	− 1	− 2	− 6	− 6	− 13
	4 PM.	− 12	− 10	− 2	− 3	− 7½	− 8	− 13
	8 PM.	− 20	− 18	− 10	− 11	− 16	− 16	− 20
7	8 AM.	− 19	− 17	− 8	− 9	− 15	− 15	− 21
	3 PM.	− 3	− 1½	+ 5	+ 4	− 0	− 0	− 8
	4 PM.	− 0	+ 2	+ 7	+ 6	+ 4	+ 4	− 3
	8 PM.	− 6	+ 2	+ 7	+ 6	+ 4	+ 4	− 3
8	Noon	+ 26	+ 28	+ 29½	+ 28½	+ 29	+ 28	+ 21
	4 PM.	+ 16	+ 18	+ 17	+ 17	+ 17	+ 17	+ 14
	8 PM.	− 0	+ 2	+ 7	+ 6	+ 4	+ 4	+ 4
9	8 AM.	− 29	− 26	− 16	− 17	− 24	− 24½	− 28
	Noon	− 14	− 12	− 4	− 5	− 10	− 10	− 16
	4 PM.	− 11	− 9	− 2	− 3	− 6½	− 7	− 13
	8 PM.	− 16	− 14	− 6	− 7	− 12	− 12	− 16

Thermometers compared.

Year, Month, and Day.	Hours.	A.	B.	D.	E.	F.	G.	H.
1782								
Mar. 10	8 AM.	−18	−16	− 7	− 6	−14	−14	−20
	1 PM.	− 4	− 2	+ 5	+ 4	+ 1	+ 1	− 5
	4 PM.	+ 1	+ 4	+ 8	+ 7	+ 5	+ 5	− 2
11	4 PM.	− 0	+ 2	+ 8	+ 7	+ 4	+ 4	− 2
	8 PM.	− 2	− 0	+ 5½	+ 6½	+ 3	+ 3	− 4
12	8 AM.	− 2	− 0	+ 5	+ 6	+ 3	+ 3	+ 4
	Noon	+ 2	+ 4	+12	+11	+10	+ 9½	+ 2
	4 PM.	+ 2	+ 4	+ 9	+ 8	+ 6	+ 6	− 0
	8 PM.	− 2	− 0	+ 5½	+ 5	+ 8	+ 7	− 6
13	8 AM.	− 4	− 2	+ 4	+ 3½	− 0	− 0	− 6
	Noon	− 0	+ 2	+ 7	+ 6	+ 4	+ 4	− 3
	4 PM.	− 0	+ 2	+ 5	+ 4	+ 2½	+ 2	− 4
	8 PM	− 7	− 5	+ 2	+ 1	− 3	− 3	−14
14	8 AM.	−18	−15	− 7	− 8	−12½	−13	−18½
	Noon	− 5	− 3	+ 3	+ 2	− 1	− 1	− 9
	4 PM.	− 6	− 4	+ 2	+ 1½	− 2	− 2	− 8
	8 PM.	−10	− 7	− 0	− 1	− 5	− 5	−10
15	8 AM.	+ 8	+ 9	+13	+12	+11	+11	+ 2
	Noon	+25	+26	+27	+26	+27	+26	+19
	4 PM.	+26	+27	+28	+27	+28	+27½	+20
	8 PM.	+26	+27½	+29	+28	+28½	+28	+20½
16	7 AM.	+ 7	+10	+13	+12	+10½	+10	+ 6
	4 PM.	+ 6	+ 8	+10	+10	− 7	− 7	− 4
	8 PM.	+ 3	+ 5	+ 9	+ 8	+ 6	+ 6	− 0
17	8 AM.	− 4	− 2	+ 4	+ 3	− 0	− 0	− 6
	1 PM.	− 0	+ 2	+ 7	+ 6	+ 4	+ 4	− 3
	4 PM.	− 0	+ 2	+ 6½	+ 6	+ 4	+ 4	− 3
	8 PM.	− 2	− 0	+ 6½	+ 5½	+ 3	+ 3	− 4
18	8 AM.	− 3	− 1	+ 5	+ 4	+ 1	+ 1	− 5
	Noon	+ 5	+ 7	+11	+10	+10	+ 9	+ 1
	4 PM.	+ 4	+ 6	+10½	+ 9½	+ 8	+ 8	+ 1
	8 PM.	− 2	− 0	+ 5	+ 4	+ 2	+ 2	− 4
19	8 AM.	− 9	− 7	− 0	− 1	− 4	− 5	−10
	Noon	− 3	− 1	+ 4½	+ 3½	+ 1	− 0	− 6
	4 PM.	− 4	− 2	+ 4½	+ 3½	− 1	− 0	− 6
	8 PM.	− 6	− 4	+ 2	+ 1	− 2	− 2	−10

Thermometers compared.

Year, Month, and Day.	Hour.	A.	B.	D.	E.	F.	G.	H.
1782								
Mar. 20	8 AM.	− 4	− 2	+ 4½	+ 3½	− 0	− 0	− 6
	4 PM.	+ 6	+ 8	+ 11½	+ 10½	+ 9	+ 8½	+ 2
	8 PM.	+ 2	+ 4	+ 9	+ 8	+ 6	+ 6	− 0
21	8 AM.	+ 3	+ 5	+ 10	+ 9	+ 7	+ 6	− 0
	Noon	+ 14	+ 16	+ 18½	+ 17½	+ 17	+ 16	+ 9
	4 PM.	+ 14	+ 16	+ 19	+ 18	+ 19	+ 18	+ 11
	8 PM.	+ 12	+ 14	+ 17	+ 16	+ 15	+ 14½	+ 9
22	Noon	+ 12	+ 14	+ 17	+ 16	+ 16	+ 15	+ 8
	4 PM.	+ 14	+ 16	+ 19	+ 18	+ 18	+ 17	+ 11
	8 PM.	+ 10	+ 12	+ 16	+ 15	+ 14	+ 14	− 8
23	8 AM.	− 6	− 4	+ 3	+ 2	− 1	− 1	− 7
	Noon	+ 4	+ 6	+ 11	+ 10	+ 9	+ 8½	− 0
	8 PM.	− 0	+ 2	+ 7	+ 6	+ 4	+ 4	− 2
24	8 AM.	− 0	+ 2	+ 8	+ 7	+ 5	+ 4	− 3

Experiment

Experiment I. made December 15, 1781.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
9 22 0	25	23		Put them into a tumbler of snow.
9 31 0	—	—		Added the spirit of nitre.
9 32 0	60	40		Thermometer defcends quick and equable.
9 32 20	88	40		
9 32 38	108	40		
9 32 55	120	40		
9 33 10	134	40		
9 33 29	150	40		
9 33 40	160	40		
9 34 0	176	40		
9 34 35	200	40		
9 34 57	214	40		
9 35 10	220	40		
9 35 50	252	40		
9 36 0	260	40		
9 36 29	280	40		
9 37 7	300	40		
9 37 48	320	40		
9 38 30	340	40		
9 39 6	352	40		
9 39 20	360	40		
9 39 48	370	40		
9 40 6	376	40		
9 40 38	384	40		
9 40 50	392	40		
9 41 19	400	40		
9 41 40	406	40		
9 42 10	414	40		
9 42 30	420	40		
9 43 0	426	40		
9 43 36	434	—		
9 44 0	438	40		
9 44 30	444	40		
9 45 0	448	40		
9 45 40	448	40		
9 54 0	448	40		

Experiment I. made December 15, 1781.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
9 55 0	448	40	—	Made a ſecond freezing mixture.
9 56 15	448	40	—	Removed the inſtruments into the ſecond mixture.
9 56 40	448	40	—	
9 57 0	448	40	—	
9 58 0	448	40	—	
9 58 40	448 1/2	40	—	
9 59 0	448 1/2	40	—	
10 0 0	448 1/2	40	—	
10 0 40	448 1/2	40	—	Added more ſpirit of nitre to the freezing mixture.
10 2 0	448 1/2	40	—	
10 3 35	448 1/2	40	—	Took the apparatus out of the freezing mixture, found it frozen, and immediately re-placed it.
10 4 14	448 1/2	42	—	
10 5 0	448	42	—	
10 7 10	448	41	—	
10 9 0	448	40	—	
10 9 40	448	40	—	
10 11 0	448	40	—	Took the apparatus out again, and endeavoured to withdraw the thermometer, but could not effect it, the quickſilver in the cylinder being frozen; put the apparatus again into the mixture.
10 11 34	448	40	—	
10 12 5	448	40	—	
10 12 20	448	40	—	
10 13 28	448	40	—	
10 15 0	—	—	—	Made a third freezing mixture.
10 16 15	444	40	—	
10 16 30	442	40	—	
10 17 10	440	40	—	
10 17 26	438	40	—	
10 17 40	—	—	—	Removed the inſtruments into the third mixture.
10 18 13	446	40	—	
10 18 40	448	40	—	
10 19 0	448	40	—	
10 20 0	448	40	—	Went away to warm myſelf.
10 26 0	448	43	—	Returned.
10 27 0	448	43	—	

Experiment

Experiment I. made December 15, 1781.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
10 27 30	448	43	—	
10 28 0	448	43	14½	Put a ſpirit thermometer (D) into the freezing mixture along with apparatus and the mercurial thermometer.
10 28 30	448	43	25	
10 28 40	448	43	28	
10 28 50	448	43	30	
10 29 0	448	43	31	
10 29 40	448	43	31½	
10 30 0	447½	42½	31½	
10 30 50	447½	42½	31	Took out the ſpirit thermometer (D).
10 31 30	447½	42½	18	Put in another ſpirit thermometer (E).
10 31 50	447½	42	24	
10 32 0	447½	42	28	
10 32 20	447½	42	31	
10 32 40	447½	42	31½	
10 32 55	448	41½	32	
10 33 20	448	41½	32½	
10 33 50	448	41½	32½	
10 34 0	448	41	32½	Took out the ſpirit thermometer (E).
10 34 50	447½	40½	—	
10 35 20	447½	40½	—	Removed the Inſtruments back into the ſecond mixture.
10 35 40	440	40	—	
10 35 49	433	40	—	
10 36 0	420	40	—	
10 36 20	410	40	—	
10 36 50	400	40	—	
10 37 8	392	40	—	
10 37 20	388	40	—	
10 37 40	380	40	—	
10 38 0	372	40	—	
10 38 20	366	40	—	
10 38 33	360	40	—	
10 40 0	324	40	—	
10 42 0	270	40	—	
10 43 0	260	40	—	
10 44 0	248	40	—	

Experiment

Experiment I. made December 15, 1781.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
10 44 15	244	40	—	Removed the inſtruments back to the 3d mixture.
10 44 35	246	40	—	
10 45 0	248	40	—	
10 46 0	247	40	—	
10 47 0	247	40	—	
10 48 0	246½	40	—	
10 49 0	246	40	—	
10 50 0	246	40	—	
10 51 0	245½	40	—	
10 52 0	245½	40	—	
10 53 0	244	40	—	
10 55 0	244	40	—	
10 55 20	244	40	—	Went away, to warm myſelf.
11 4 0	114	38	—	Returned.
11 9 0	54	37	—	
11 9 50	48	37	—	
11 11 0	40	36	—	
11 12 0	39	35½	—	Put in the ſpirit thermometer (D).
11 13 0	—	—	24	
11 13 40	38	35½	—	
11 14 0	38	—	26½	Took out the apparatus; the quickſilver was perfectly fluid, and the incloſed thermometer (F) was eaſily withdrawn.

Remarks and obſervations on the firſt experiment.

Finding the thermometer on Thurſday evening, the 14th of December, was 18° below the cypher, I concluded the morning would afford me an opportunity to make an attempt to fix the point at which quickſilver begins to freeze; I therefore put a bottle of ſpirit. nitri fortis upon the top of the houſe in open air, that it might be of the ſame temperature when it was to be

uſed. The thermometer had been hung up before, three weeks, in the open air, to compare their ſcales. At 7 o'clock in the morning of the 15th, the thermometers were about 23° below nought; I therefore made preparations for the experiments, getting the quickſilver out into the air, providing glaſs tumblers for mixing the nitrous acid with the ſnow, &c. I put as much quickſilver into a glaſs cylinder as (when the thermometer (F) was introduced) juſt filled the bulbous part of the cylinder; the ſcale of the thermometer did not reach the length of the tube by about three inches; and the bare part of the tube was wound round with red worſted in two places, to a thickneſs ſufficient to fill the upper part of the orifice of the cylinder in order to exclude the external air: now, as the quickſilver only filled the bulb, there was a ſpace of near half an inch left empty between the quickſilver and the neareſt piece of worſted, ſo that, by inclining the apparatus, the quickſilver readily ran out of the bulb into the other part of the cylinder. This was done with an intention to diſcover the more eaſily when the quickſilver ceaſed to be fluid; for, by taking the inſtrument out of the freezing mixture, and elevating the lower end, the quickſilver, if not frozen, would run into the void ſpace.

The experiment was made in the open air, on the top of the Fort, with only a few deer-ſkins ſewed together, placed to windward for a ſhelter: there was plenty of ſnow (eighteen inches deep) upon the works, and the thermometers were cloſe at hand. In thruſting the thermometer (F) into the quickſilver, the inſtrument roſe to the cypher, but ſoon began to deſcend again; but being unwilling to loſe time, I ſtuck the apparatus into the ſnow, the ſooner to bring it to the temperature of the air.

The

The table will fully explain the process. I was in hopes, by shifting the instruments into three fresh mixtures, I should have been able to have produced a greater degree of cold than by one only; yet it did not. I added more spirit of nitre, but without effect. At 10 h. 3′ 35″ I took out the apparatus, and raised the bulbous end to make the quicksilver run, but found it was frozen, so that it did not alter its figure in the least. I then placed it in the mixture, where it contined till 10 h. 11′, when I made another trial as before, but without perceiving any alteration: however, to be more certain of its being frozen, I proposed to take out the thermometer; but all the strength in my fingers could not move it in the least, so that myself and officers, who stood by, were convinced it was frozen fast. I then made another mixture in hopes to augment the cold, and make the inclosed thermometer (F) descend; however, seeing no alteration, I went into the house to warm myself, and on my return found it had fallen 3°. I tried the coldness of the mixture by different spirit thermometers, and afterwards shifted the instruments into the mixture from whence I had taken them; but this diminished the cold by the thermometer, so that I re-placed them again in the third mixture, and the quicksilver in the thermometer descended again to its former point 448°. I continued observing it some minutes, when the cold obliged me a second time to retire, and on my return found both the thermometer and apparatus rising: on dipping a spirit thermometer into the mixture, I found it had a considerable degree of coldness, and both the apparatus and mercurial thermometers were nearly equal. I then took them out, and the quicksilver in the cylinder was as fluid as when it was first poured in.

I should have observed, that during the time I was pouring in the spirit of nitre at the beginning of the operation, I was

ſo engaged in mixing it with the ſnow, that I did not ſee the thermometer in the apparatus ſink to 40°, which muſt have been very ſudden, becauſe I was but one minute before I obſerved it. I could obſerve no alteration in the quickſilver in the cylinder when it was frozen, and intending to make more experiments, I was unwilling at this time to break the glaſs.

The time was taken by a good watch which ſhews ſeconds, and (which however I apprehend can be of little conſequence) about 5′ 10″ too faſt by apparent time. I had two aſſiſtants; one to repeat audibly every ſecond, and the other to write down the time and the obſervations as faſt as I made them.

The obſervations were taken down with a pencil, but copied fair with ink into my note book: they were compared the ſame day the experiment had been made, to avoid miſtakes; and theſe remarks were written at the ſame time, whilſt the remembrance of them was yet ſtrong on the mind.

The thermometers uſed on this occaſion were thoſe marked A and F.

Experiment

Experiment II. made Dec. 16, 1781.

Time per Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
8 19 30	34	31	—	Put the inſtruments into a tumbler of ſnow.
8 21 30	—	—	—	Began to pour in the ſpirit. nitri fortis.
8 21 45	40	34	—	
8 22 12	40	36	—	
8 22 40	40	36	—	
8 23 0	32	32	—	A large proportion of ſpirit of nitre poured in.
8 23 31	38	29	—	Adding ſnow to the mixture, it being too thin.
8 23 50	40	30	—	
8 24 0	43	34	—	
8 24 15	44	36	—	
8 24 40	58	40	—	
8 24 50	66	43	—	
8 25 0	76	43	—	
8 25 19	80	40	—	
8 25 29	84	40	—	
8 25 46	90	40	—	
8 26 0	94	40	—	
8 26 12	100	40	—	
8 26 24	104	40	—	
8 26 43	110	40	—	
8 27 0	116	40	—	Added more ſnow, the quantity of the mixture being ſmall.
8 27 22	126	40	—	
8 27 42	138	40	—	
8 28 0	146	40	—	
8 28 29	158	40	—	
8 28 45	164	40	—	
8 29 0	168	40	—	
8 29 25	176	40	—	
8 29 52	180	40	—	
8 30 20	184	40	—	Found the mixture did not cover the bulb of the mercurial thermometer.
8 30 50	210	40	—	Poured in more ſpirit of nitre.
8 31 20	160	40	—	
8 31 28	156	40	—	Put in ſnow by degrees, and ſtirred the mixture.
8 31 43	148	40	—	Ditto,
8 32 0	152	40	—	

Experiment II. made December 16, 1781.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
8 32 10	156	40	—	
8 32 20	162	40	—	
8 32 31	168	40½	—	
8 32 48	178	42½	—	
8 33 0	188	43	—	
8 33 15	194	44	—	
8 33 26	200	44½	—	
8 33 39	206	92	—	The mercury in the apparatus thermometer ſunk ſo inſtantaneouſly, I could not catch any intermediate degrees.
8 33 47	206	95	—	
8 34 0	206	95	—	
8 34 21	206	95	—	
8 34 35	206	95	—	
8 35 0	206	95	—	
8 35 30	206	95	—	
8 36 0	206	95	—	
8 36 30	206	95	—	Put in the ſpirit thermometer D.
8 37 0	206	95	30	
8 37 30	206	95	32	
8 38 0	206	95	33	
8 38 30	206	95	33	
8 39 0	206	95	33	Made a freſh freezing mixture.
8 40 0	206	95	33	Removed all the inſtruments into it.
8 41 0	206	95	33	
8 42 0	206	95	33	
8 43 0	206	95	33	
8 44 0	206	95	33	
8 45 0	206	95	33	
8 46 0	206	95	33	
8 46 30	206	95	33	Took out the apparatus, and found the quickſilver in the cylinder frozen; replaced it.
8 46 54	206	95	33	
8 47 0	206	95	33	From 8 h. 47′ to 9 h. 11′ employed in making the next experiment.
8 48 0	206	95	33	

Experiment II. made December 16, 1781.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
9 11 0	206	95	32½	Went to breakfast, and, seeing no alteration, intended only to return now and then, it being Sunday, and prayer time being at hand.
9 50 0	206	95	31½	
10 35 0	378	Bulb	27	
10 37 10	348	D°	—	
10 38 0	320	D°	—	Took the apparatus out to examine it; then put it in again.
10 38 40	310	D°	—	
10 39 30	306	D°	—	
10 40 30	296	D°	—	
10 41 20	290	D°	27	
10 41 50	284	D°	—	
10 42 15	280	D°	27	
10 43 0	273	D°	—	
10 43 50	260	D°	—	
10 44 50	250	D°	—	
10 46 10	234	D°	—	
10 46 40	227	D°	—	
10 47 0	222	D°	26	
10 47 30	216	D°	26	
10 48 15	206	D°	—	
10 48 35	202	D°	—	
10 49 10	194	D°	—	
10 49 30	189	D°	26	
10 50 0	182	D°	—	
10 50 20	177	D°	—	
10 50 50	170	D°	26	
10 51 15	164	D°	—	Stirred the instruments about in the mixture.
10 51 40	156	D°	26	
10 52 20	147	D°	—	
10 52 50	141	D°	—	
10 55 5	112	D°	25¼	
10 55 30	107	D°	—	
10 56 0	102	D°	—	
10 56 15	98	D°	25	
10 56 35	94	D°	—	
10 56 50	91	D°	—	

Experiment II. made December 16, 1781.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
10 57 10	86	Bulb	—	
10 57 30	82	D°	—	
10 57 45	79	D°	25	
10 58 5	75	D°	—	
10 58 30	70	D°	—	
10 58 45	67	D°	—	
10 59 10	62	D°	—	
10 59 30	58	D°	—	
11 0 0	53	D°	—	
11 0 15	50	D°	—	
11 0 40	47	D°	24½	
11 1 0	44	D°	24½	
11 1 20	41	D°	—	
11 2 0	40	D°	—	
11 2 30	39	D°	—	
11 3 0	39	D°	—	
11 3 30	39	D°	—	
11 4 0	38½	D°	—	The mercury in the apparatus thermometer raising up the tube from the bulb.
11 4 20	38	235	—	
11 4 30	38	225	—	
11 4 40	38	220	—	
11 4 50	38	218	—	
11 5 0	38	205	—	
11 5 10	38	195	—	
11 5 20	38	183	—	
11 5 33	38	172	—	
11 5 44	38	163	—	
11 5 55	38	154	—	
11 6 10	38	140	—	
11 6 20	38	130	—	
11 6 30	37	120	—	
11 6 40	37	110	—	
11 6 50	37	97	—	
11 7 0	37	87	—	
11 7 10	37	78	—	
11 7 20	37	67	—	

Experiment

Experiment II. made December 16, 1782.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
11 7 40	37	47	—	
11 8 0	37	38	—	
11 8 20	37	38	—	
11 8 40	37	38	23½	
11 9 15	37	37½	—	
11 9 30	37	37	—	Examined apparatus, quickſilver in the cylinder perfectly fluid.
11 15 0	36	35	23	

This experiment was made with the ſame inſtruments as the preceding, and the quickſilver which was left yeſterday in the cylinder was the ſame now employed. I was rather unfortunate in making too ſmall a quantity of the freezing mixture at the beginning, which obliged me to make repeated additions to it: by this means the operation was not only retarded, but ſometimes it even undid what had been done; for in pouring in the nitrous acid it was unavoidable but part of it ſhould come in contact with the bulbs of the inſtruments before it was mixed with ſnow. In this caſe it never failed making the thermometers riſe ſuddenly much higher than where they ſtood before the ſpirit was added; and at length it only deſcended to 206°, which is not half ſo low as on the preceding day, though the temperature of the air was ten degrees colder (*viz.* 34°): yet it is remarkable, that though the thermometer was ſo much higher, the apparatus was ſunk more than twice as low as the day before; for after having been long ſtationary at 40°, it ſunk

to 95°. I then made a freſh mixture, but it had no effect any way during three quarters of an hour I attended to it afterwards. During this idle interval I made the third experiment. Finding no alteration, I went down to breakfaſt, and on my return was ſurpriſed to find the quickſilver in the apparatus thermometer had ſubſided into the bulb, and the ſtandard thermometer had been very low (how low I cannot tell), and was riſing briſkly. The ſpirit thermometer alſo ſhewed the mixture had a leſs degree of cold than before. To be certain that the quickſilver in the apparatus thermometer was in the bulb, I took the apparatus out of the mixture, and examined it minutely for half a minute, till I was quite certain of it; and alſo that the quickſilver in the cylinder was frozen, and it is remarkable, it did not liquify in all that time.

The obſervations were made with the greateſt attention, and (on every particular occaſion) noted down as quick as poſſible.

Experiment

Experiment III. made December 16, 1781.

Time per Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″	B.	G.	E.	
8 55 55	34	35	—	Put them into the first freezing mixture used in the preceding experiment.
8 56 10	40	40	—	
8 56 21	41	41	—	
8 56 42	42	42	—	
8 57 0	43	42½	—	
8 57 26	43	43	—	
8 58 0	43	43	—	
8 58 58	54	43	—	
8 59 14	60	43	—	
8 59 40	64	43	—	
9 0 0	68	43	—	
9 0 34	70	43	—	
9 1 0	75	43	—	
9 1 22	78	43	—	
9 2 0	82	43	—	
9 2 30	85	43	—	
9 3 0	88	43	—	
9 4 0	94	43	—	Put the spirit thermometer (E) into the mixture.
9 4 21	94	43	27	
9 4 40	98	43	30	
9 5 12	100	43	31½	
9 6 0	104	43	32	
9 6 30	106	43	32	
9 7 0	108	43	32	
9 7 40	110	43	32	
9 8 20	112	43	—	
9 9 0	114	43	—	
9 10 30	116	40	—	The quickfilver in the apparatus was fluid, but seemed thick and in grains, somewhat like crumbs of bread; replaced it again, and went to breakfast.
9 50 0	40	40	—	Examined the quickfilver again, it was frozen hard.
10 55 40	—	37½	—	The quickfilver as fluid as ever.

This experiment was made during the continuance of that which immediately precedes it, as may be seen by examining

the time by the watch, and was the effect of chance; for the firſt freezing mixture, which had been uſed in the ſecond experiment, ſtanding in the glaſs cloſe to me (and the other inſtruments being long ſtationary, did not require particular attention), I took down the thermometer (G) and charged its cylinder with quickſilver, as in the other examples, and ſuſpended it in the old mixture, together with the mercurial thermometer (B) and a ſpirit thermometer; the mixture ſeemed to have loſt much of its coldneſs, as appeared by the thermometers. It ſeemed very extraordinary to me, that the apparatus, after having been ſo long ſtationary at 43°, ſhould yet contain fluid quickſilver; but both myſelf and aſſiſtant thought it was thicker than ordinary, as it did not run freely, but ſeemingly in pieces (not globules): however we put it back again into the mixture, and ſet it by as of no further uſe; but returning after breakfaſt, we found it was firmly frozen, ſo as to give no appearance of fluidity though the included thermometer was only at 40°, which I look upon to be the exact freezing point of quickſilver; and then the congelation was in fact begun before, and effected by only a longer continuance in the ſame degree of cold.

It may be neceſſary to mention, that the ſpace between the bottom of the ivory ſcale to the bulb of the thermometer (F) which made part of the apparatus uſed in the ſecond experiment, was two inches nine-tenths; and when taken with a pair of compaſſes (dividers) with one foot placed at the cypher o on the graduated ſcale, the other extended to 148° if meaſured upwards, and to 165° if meaſured downwards, for the diviſions were unequal.

Experiment

Experiment IV. made January 7, 1782.

Time *per* watch.	Therm. below 0.	Apparatus.	Spirit Therm.	Remarks and Occurrences.
h. ′ ″				
8 7 0	35	30	—	Put the inſtruments into a tumbler of ſnow.
8 9 45	35½	27	—	
8 10 40	—	—	—	Added the ſpirit of nitre.
8 11 0	42	42	—	Added more ſnow to increaſe the quantity of the mixture.
8 12 30	—	—	—	Added more ſpirit.
8 13 15	68	42	—	
8 13 35	78	42	—	
8 14 0	92	42	—	
8 14 15	100	42	—	
8 14 30	108	42	—	
8 14 45	116	42	—	
8 15 0	126	42	—	
8 15 15	134	42	—	
8 15 30	140	42	—	
8 15 45	146	41½	—	
8 16 20	160	41½	—	
8 16 30	166	41½	—	
8 16 45	171	41½	—	
8 17 15	182	41½	—	
8 17 45	191	41½	—	
8 18 0	196	41½	—	
8 18 15	202	41½	—	
8 18 30	208	41½	—	
8 18 45	212	41½	—	
8 19 30	232	77	—	The deſcent in the apparatus therm. very quick.
8 19 40	240	77	—	
8 20 30	258	77½	—	
8 20 40	262	77½	—	
8 21 0	270	77½	—	
8 21 15	278	77½	—	
8 21 45	280	77½	—	
8 22 0	286	77½	—	
8 22 30	293	77½	—	
8 22 35	298	77½	—	
8 23 0	302	78	—	
8 23 15	305	78	—	

Experiment IV. made January 7, 1782.

Time *per* Watch.	Thermom. below 0.	Appara-tus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
8 23 30	309	78	—	
8 24 0	316	78	—	
8 24 15	319	78	—	
8 24 30	322	78	—	
8 24 45	327	78	—	
8 25 0	329	78	—	
8 25 15	332	78	—	
8 25 30	336	78	—	
8 25 45	338	78	—	
8 26 0	342	78	—	
8 26 15	345	78	—	
8 26 30	348	78	—	
8 26 45	351	78	—	
8 27 0	354	78	—	
8 27 15	356	78	—	
8 27 30	359	78	—	
8 27 45	361	78	—	
8 28 0	364	78	—	
8 28 15	366	78	—	
8 28 30	368	78	—	
8 28 45	370	78	—	
8 29 0	373	77	—	
8 29 15	376	77	—	
8 29 30	378	77	—	
8 29 45	380	77	—	Stirred the mixture.
8 30 15	386	77	—	
8 30 30	388	77	—	
8 31 0	392	77	—	
8 31 15	394	76½	—	
8 31 30	396	76½	—	
8 31 45	398	76½	—	
8 32 15	402	76½	—	
8 32 30	403	76	—	
8 32 45	404	76	—	
8 33 0	406	76	—	
8 33 15	408	76	—	Put in ſpirit thermometer (P).
8 34 0	412	76	31½	

Experiment IV. made January 7, 1782.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
8 34 30	416	76	32	
8 35 0	418	76	32	
8 35 30	422	76	32	
8 36 0	424	76	32	
8 36 15	425	76	32	
8 36 30	425	76	32	
8 36 45	427	76	32	
8 37 0	428	76	32	
8 37 15	429	76	32	
8 37 30	430	76	32	
8 37 45	430	76	32	
8 38 0	431	76	32	
8 38 30	432	76	32	
8 38 45	433	76	32	
8 39 0	434	76	32	
8 39 15	435	76	32	
8 39 30	435½	76	32	
8 40 0	436	76	32	
8 40 30	436	76	32	
8 40 45	437	76	32	
8 41 15	438	76	32	
8 41 30	438½	76	32	
8 41 45	439	76	31½	
8 42 15	440	76	31½	
8 43 0	440	76	31½	
8 43 15	440	75½	31½	
8 44 30	440	75½	31	Made a fresh mixture.
8 47 0	438	75	31	Removed the instr. into the new freezing mixture.
8 47 36	448	76	34½	
8 48 0	448	76½	35	
8 48 30	448	77	35	
8 49 0	448	77	35	
8 49 30	449	77	35	
8 50 0	449	77	35	
8 50 30	450	77	35	
8 51 0	450	77	35	
8 52 0	450	77	35	

Experiment IV. made January 7, 1782.

Time per Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
8 53 0	450	77	35	
8 54 0	450	77	35	
8 56 0	450	77	35	
8 57 0	450	77	35	
8 58 0	450	77	35	
8 59 0	450	77	35	
9 0 0	450	77	35	
9 1 0	450	77	35	
9 2 0	450	77	35	
9 3 0	450	77	35	
9 4 0	450	77	35	
9 5 0	450	77	35	
9 6 0	450	77	35	
9 7 0	450	77	35	Examined the apparatus; found all solid.
9 8 0	450	77	35	
9 9 0	450	77	35	
9 10 0	450	77	35	
9 11 0	450	77	35	
9 12 0	450	77	35	
9 13 0	450	77	35	
9 14 0	450	77	35	
9 15 0	450	77	35	
9 16 0	450	77	35	
9 17 0	450	77	35	
9 18 0	450	77	35	
9 19 0	450	77	35	
9 20 0	450	77	35	
9 21 0	450	77	35	
9 22 0	450	77	35	
9 23 0	450	77	35	Added more snow.
9 23 30	450	77	35	
9 24 0	450	77	34⅝	
9 25 0	450	77	34½	
9 26 0	450	77	34½	
9 27 0	450	77	34½	
9 28 0	450	77	34½	

Experiment

Experiment IV, made January 7, 1782.

Time *per* Watch.	Thermom. below 0. Appara-tus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″			
9 29 0	450 77	34½	
9 30 0	450 77	34½	
9 31 0	450 77	34½	
9 32 0	450 77	34½	
9 33 0	450 77	34½	
9 34 0	450 77	34½	
9 35 0	450 77	34½	
9 36 0	450 77	34¼	
9 37 0	449 77	34	
9 38 0	449 77	33½	
9 39 0	449 77	33	
9 40 0	449 77	33	
9 41 0	449 77	32½	
9 42 0	449 77	32½	
9 43 0	448½ 76	32½	
9 44 0	448½ 76	32½	
9 45 0	448½ 76	32½	
9 46 0	448½ 76	32	
9 47 0	448½ 76	32	
9 48 0	448½ 76	32	
9 49 0	448½ 76	32	
9 50 0	448½ 76	31½	
9 51 0	448½ 76	31½	
9 52 0	448 75½	31½	
9 53 0	448 75½	31½	
9 54 0	448 75	31¼	
9 56 0	448 75	31	
9 57 0	447 75	31	
9 58 0	445 75	30½	
9 58 30	444 75	30½	
9 58 45	443 75	30½	
9 59 0	442 75	30½	
9 59 15	441 75	30½	
9 59 30	440 75	30½	
9 59 45	439 75	30½	
10 0 0	438 75	30½	
10 0 15	437 75	30½	

Experiment IV. made January 7, 1782.

Time *per* Watch.	Thermom. below 0.	Appara-tus.	Spirit Thermom.	Remarks and observations.
h. ′ ″				
10 0 30	436	75	30½	
10 0 45	435	75	30½	
10 1 0	434	75	30½	
10 1 15	433	75	30½	
10 1 30	432	75	30¼	
10 1 45	431	75	30¼	
10 2 0	430	75	30	
10 2 15	429	75	30	
10 2 45	426	74	30	
10 3 30	422	74	30	
10 4 15	417	74	30	
10 4 45	414	74	30	
10 5 0	412	74	30	
10 5 30	408	73½	30	
10 6 0	405	73½	30	
10 6 30	401	73½	29½	Filled up the tumbler with a former mixture.
10 7 30	388	73	28½	
10 8 0	382	74	28	
10 8 30	377	82	28	
10 8 46	374	87	28	
10 9 0	368	110	28	
10 9 10	366	125	28	
10 9 20	364	170	28	
10 9 40	360	200	28	
10 9 50	358	240	28	
10 10 0	356	Bulb	28	
10 10 30	350	D°	28	
10 11 10	340	D°	28	
10 12 0	332	D°	28	
10 12 30	327	D°	28	
10 13 0	321	D°	28	
10 13 30	316	D°	28	
10 14 0	311	D°	28	
10 14 30	306	D°	28	
10 15 0	301	D°	28	
10 15 30	296	D°	28	Made a fresh mixture.
10 16 0	—	D°	28	Put the instruments into a fresh mixture.

Experiment

Experiment IV. made January 7th 1782.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
10 16 30	312	Bulb	29	
10 17 0	352	D°	35	
10 17 45	373	D°	35	
10 18 0	384	D°	35	
10 18 15	389	D°	35	
10 18 30	389	D°	35	
10 18 45	389	D°	35	
10 19 0	389	D°	35	
10 20 0	389	D°	35	
10 20 30	389	D°	35	
10 21 0	389	D°	35	
10 22 0	389	D°	35	
10 23 0	389	D°	35	
10 24 0	389	D°	35	
10 25 0	389	D°	35	
10 26 0	389	D°	35	
10 27 0	389	D°	35	
10 28 0	389	D°	35	
10 29 0	389	D°	35	
10 30 0	389	D°	35	
10 31 0	389	D°	35	
10 32 0	389	D°	35	
10 33 0	389	D°	35	
10 34 0	389	D°	35	
10 35 0	389	D°	35	
10 36 0	389	D°	35	
10 37 0	389	D°	35	
10 38 0	389	D°	35	
10 39 0	389	D°	35	
10 40 0	389	D°	35	
10 41 0	389	D°	35	
10 42 0	389	D°	35	
10 43 0	389	D°	35	
10 44 0	389	D°	34½	
10 45 0	389	D°	34½	
10 46 0	389	D°	34½	

Experiment IV. made January 7, 1782.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
10 47 0	389	Bulb	34½	
10 48 0	389	D°	34½	
10 49 0	389	D°	34½	
10 50 0	389	D°	34	
10 51 0	389	D°	34	
10 52 0	389	D°	34	
10 53 0	288	D°	34	
10 55 0	288	D°	33½	
10 59 0	288	D°	33¼	
11 1 0	288	D°	33	
11 5 0	388	D°	32½	
11 10 0	388	D°	32	
11 15 0	444	D°	31½	
11 15 10	446	D°	31	
11 15 30	445	D°	31	
11 15 45	444	D°	31	
11 16 15	443	D°	31	
11 17 0	441	D°	31	
11 18 0	437	D°	31	
11 19 0	434	D°	30	
11 20 0	428	D°	30	
11 21 30	418	D°	29½	Whilſt examining the apparatus, part of the quickſilver turned fluid.
11 22 30	412	D°	29½	
11 23 0	408	D°	29½	
11 23 30	404	D°	29¼	
11 24 0	400	D°	29	
11 24 30	396	D°	29	
11 25 30	390	D°	29	The center of the quickſilver appeared globular and ſolid.
11 26 30	379	D°	29	
11 28 0	365	D°	29	
11 29 0	355	D°	29	
11 29 30	352	D°	29	
11 29 45	349	D°	29	
11 30 0	346	D°	29	

Experiment IV. made January 7, 1782.

Time *per* Watch.	Thermom. below o.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
11 30 15	344	Bulb	29	
11 30 30	342	D°	29	
11 30 45	339	D°	29	
11 31 0	336	D°	29	
11 31 15	333	D°	29	
11 31 30	330	D°	29	
11 31 45	328	D°	29	
11 32 0	325	D°	29	
11 32 30	320	D°	29	
11 32 45	318	D°	29	
11 33 0	314	D°	29	
11 33 15	312	D°	29	
11 33 30	309	D°	28¼	
11 33 45	306	D°	28¼	
11 34 0	303	D°	28¼	
11 34 15	301	D°	28¼	
11 34 30	298	D°	28¼	
11 34 45	295	D°	28¼	
11 35 0	292	D°	28¼	
11 35 15	289	D°	28¼	
11 35 30	286	D°	28¼	
11 35 45	283	D°	28¼	
11 36 0	280	D°	28¼	
11 36 15	277	D°	28¼	
11 36 30	274	D°	28¼	
11 37 0	267	D°	28¼	
11 37 15	264	D°	28¼	
11 37 30	262	D°	28¼	
11 37 45	258	D°	28¼	
11 38 0	255	D°	28	
11 38 15	252	D°	28	
11 38 30	249	D°	28	
11 38 45	246	D°	28	
11 39 0	241	D°	28	
11 39 30	237	D°	28	
11 39 45	234	D°	28	

Experiment IV. made January 7, 1782.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
11 40 0	231	D°	28	
11 40 30	224	D°	28	
11 41 0	216	D°	28	
11 41 30	211	D°	28	
11 42 0	204	D°	27½	
11 42 30	197	D°	27½	
11 43 0	190	D°	27½	
11 43 30	185	D°	27½	
11 44 0	178	D°	27½	
11 44 30	170	D°	27	
11 45 0	160	D°	27	
11 45 30	150	D°	27	
11 46 0	142	D°	27	
11 46 30	134	D°	27	
11 47 0	125	D°	27	
11 47 30	118	D°	27	
11 48 0	109	D°	27	
11 48 30	103	D°	27	
11 49 0	96	—	27	The quickfilver in the apparatus thermometer rifing faft in the tube.
11 49 15	90	227	27	
11 49 30	88	215	27	
11 49 40	86	205	27	
11 49 50	84	193	27	
11 50 0	82	180	27	
11 50 10	80	170	27	
11 50 20	78	157	27	
11 50 30	73	145	27	
11 50 40	73	132	27	
11 50 50	72	120	27	
11 51 0	69	105	27	
11 51 10	68	88	27	
11 51 20	64	65	27	
11 51 35	62	45	27	
11 51 50	59	37½	27	
11 52 0	56	38	27	

Experiment

Experiment IV. made January 7, 1782.

Time per Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
11 52 30	53	38	27	
11 53 0	40	38	27	
11 53 30	42	37	27	Found the quickſilver wholly thawed in the cylinder
11 54 0	40	37	26½	
11 54 30	39	37	26½	
11 55 0	39	37	26½	
11 55 30	39	37	26½	
11 56 0	39	37	26½	
11 56 30	38	36	26	

This experiment was made with the mercurial thermometer (A) and the apparatus (F) as in the firſt and ſecond experiments. The day was clear, with little wind at W. by S. or W.S.W. which I have obſerved to be generally the caſe in this country in the coldeſt weather. The thermometers at 8 o'clock were as follows, according to the rotation of the letters from (A) to (G), 39°½, 36°½, 35°, 25°, 25°, 34°½, 34° below the cypher. The apparatus thermometer (F), after ſtanding at 42° and 41°¼ for a conſiderable time, ſunk at once to 77°, not gradually or by jumps, but ſuddenly, as a weight falleth. The great deſcent of the quickſilver in the index thermometer (A) to 440° in the firſt freezing mixture I impute to the coldneſs of the weather, but was ſurpriſed to find it did not ſink more than 10° lower in the ſecond mixture; and in the third it did not reach ſo low as in the preceding, which, indeed, might be accounted for by the air growing warmer as the ſun approached the meridian. At 10 h. 6′¼ I poured ſome of the firſt mixture into the tumbler where the inſtruments were immerſed in the ſecond, but found it weakened it; I therefore mixed a freſh one at 10 h. 16′. It is however remarkable, that after pouring in the firſt mixture

on the ſecond, the apparatus, which had riſen a little before, ſunk ſuddenly into the bulb. I have marked its progreſs as faſt as I could catch it. Another extraordinary circumſtance in this experiment is, that the mercurial thermometer (A) ſhould not ſubſide lower in the third than in the ſecond mixture; whereas the ſpirit thermometer ſhewed an equal degree of cold, while the quickſilver in the apparatus thermometer was in the bulb. At 11 h. 21′ I took the apparatus out to examine it, and, by ſhaking it in my hand, all of a ſudden ſome of the quickſilver in the cylinder liquified; the concuſſion perhaps diſſolved its ſolidity, for it was not above a minute out of the mixture. Wondering much at this unexpected phænomenon, as the quickſilver in the thermometer did not riſe, I put it into the mixture again immediately; but finding the incloſed thermometer ſhewed no alteration, my curioſity determined me to examine it again; therefore, about four minutes after, I took it out a ſecond time, and found the ſurface of the quickſilver in the cylinder was liquified about one-eighth of the whole quantity, as near as I could gueſs; the reſt formed a ſolid ball, including the bulb of the thermometer, which eaſily accounts for the quickſilver in that inſtrument remaining ſtationary. Wiſhing to obſerve the whole proceſs, and the cold being too ſevere for the ſame perſons to ſtand in the open air for ſo long a time, I deſired one of my officers, with an aſſiſtant, to mark down the obſervations at the times I went to warm myſelf, but by no means to make any alterations in my abſence; by this means the obſervations were continued regularly for near four hours.

At the end of the experiments the thermometers (B), (C), (D), and (G), ſtood as follows, 18°½, 15°, 9°½, 15, which ſhews the alteration in the temperature of the air from the beginning. The thermometers (A), (D), and (F), were uſed in the experiment.

Experiment

Experiment V. made February 22, 1781.

Time *per* Watch.	Thermom. below 0.	Apparatus.	Spirit Thermom.		Remarks and Occurrences.
h. ′ ″	A.	G.	C.		
8 4 0	78	40	29½		
8 9 0	89	40	29		Making the freezing mixture.
8 11 0	—	—	—		Put the inſtruments into the mixture.
8 11 10	70	40	31½		
8 11 30	60	39½	32		
8 11 50	54	39½	32		
8 12 30	—	—	—		Added more ſnow.
8 13 0	50	41	33½		
8 13 30	51	42	34		
8 14 0	51	72	34		
8 14 15	51	78	34		
8 14 32	51	78	34		
8 15 0	—	—	—		Added more ſnow.
8 15 20	52	78	35		
8 15 50	—	—	—		Added more ſnow.
8 16 10	52	79	35		The quickſilver in the cylinder fluid.
8 17 0	52	79	35		
8 20 0	52	79	35		
8 25 0	52	79	35		Made a freſh mixture.
8 30 0	52	79	35		Removed the inſtruments into the new mixture.
8 30 45	52	79	35		
8 31 30	52	79	35		The quickſilver in the cylinder ſtill fluid.
8 32 30	52	79	35	64	Put in mercurial thermometer (B).
8 32 50	52	79	35	80	
8 33 20	52	79	35	90	
8 34 0	52	79	35	90	
8 35 0	52	79	35	91	
8 37 0	52	79	35	91	The quickſilver in the cylinder ſolid frozen.
11 31 0	52	79	35	91	Took out all the inſtruments.

Remarks on the fifth experiment.

I had not intended to make any more experiments of this kind, thinking thoſe already made had fully determined the freez-

ing point of quickſilver; but the arrival of a gentleman, who wiſhed to ſee it, induced me to repeat it again. The weather was clear and ſerene, the wind about S.S.W., and the ſeveral thermometers ſtood as follows, A 82°, B 66°, D 34°, E 34°, F 42°, G 42°, H 46°, at ſeven o'clock in the morning; and at eight o'clock they were A 78°, B 114°, D 29°½, E 29°½, F 29°½, G 40°, H 43°; yet it is remarkable, that quickſilver which was conſtantly expoſed to the air in a ſaucer was not froze. I impute the ſmall deſcent of the quickſilver in the thermometers to the great degree of the cold in the atmoſphere as in the ſixth experiment, for there the effect was ſimilar. The moſt remarkable circumſtance in this day's operation was the ſudden deſcent of the quickſilver in the apparatus thermometer, and the length of time it continued at 79° before the quickſilver in the cylinder became ſolid. The freezing mixture retaining an equal degree of cold for ſo long a time (as appeared by the ſpirit thermometer), and the conſequent ſtationary ſituation of all the inſtruments, I apprehend, proceeded from the continual cold in the circum-ambient air; for at one o'clock the thermometers were riſen but very little, being as follows, A 35°, B 32°, D 21°, E 22°, F 30°, G 30°, H 34°, the wind blowing briſk from N.W.

Experiment

Experiment VI. made January 11, 1782.

Time per Watch.	Standard ſpirit H.	Mercurial therm. C.	Spirit therm. D.	Remarks and Occurrences.
h. ′ ″				
8 15 0	42	50	27½	The inſtruments juſt put into the freezing mixture.
8 15 50	43	70	33	
8 16 15	43	100	34	
8 16 45	44	120	34	
8 17 15	44	140	34	
8 17 30	44	150	34	
8 17 45	44	155	34	
8 18 30	45	170	34	The mixture did not cover the bulb of (H).
8 19 0	—	—	—	Added more ſnow and ſpirit.
8 20 20	44	170	34	
8 21 0	45	170	34	
8 22 15	46	170	33	
8 22 50	46	170	33½	
8 23 30	46	170	33½	
8 24 30	46	170	33½	
8 25 10	46	171	33½	
8 26 0	46	171	33½	
8 27 10	46	172	33½	
8 27 45	46	172	33½	
8 29 30	46	172	33½	Added more ſnow.
8 30 30	46	172	33½	
8 35 0	46	172	33½	Made a freſh mixture.
8 36 0	46	173	34½	Put the inſtruments into a freſh mixture.
8 36 30	46	173	34½	
8 37 0	46	173	34½	
8 37 30	46½	173	34½	
8 38 30	47	174	35	
8 39 40	47	174	35	
8 40 30	47½	174	35	
8 41 15	48	174	35	
8 42 0	48	175	35	
8 43 0	48	175	35	
8 44 0	48	175	35	
8 45 0	48	175	35	
8 45 15	47½	175	35	
8 46 0	47½	175	35	
8 46 30	47½	175	35	

Experiment VI. made January 11, 1782.

Time *per* Watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. C.	Mercurial th. A and B.	Remarks and Occurrences.	Mer. ther. A in air.
h. ′ ″				A.		
8 47 30	47½	174	34½			
8 48 0	47½	174	34½	45	Put mercurial th. (A) into the mixture.	
8 50 0	47½	174	35	76		
8 50 30	47½	174	35	114		
8 50 45	47½	174	35	124		
8 51 0	47½	174	35	146		
8 51 30	47½	174	35	147		
8 52 30	47½	174	35	147		
8 53 30	47½	174	35	147½		
8 54 0	47½	174	35	147½		
8 55 0	47½	174	35	147½		
8 56 0	47½	174	35	147½		
8 56 30	47½	174	35	147½		
8 57 35	47½	175	35	148		
8 58 0	48	175	35	148		
8 59 0	48	175	35	148		
9 0 0	48	175	35	148		
9 1 0	48	175	35	148		
9 2 0	48	175	35	148		
9 3 0	48	175	35	148		
9 4 0	48	175	35	148		
9 5 0	48	175	35	148	Took out thermometer (A).	148
				B.		
9 8 0	48	174	35	70	Put in mercurial thermometer (B).	148
9 9 45	48	174	35	84		148
9 10 15	48	174	35	86		148
9 11 0	48	174	35	86	Added more snow.	148
9 13 0	48	173	35	86		148
9 15 0	47	173	34½	86		148
9 16 0	47	173	34½	86		148
9 17 0	46½	173	34½	86		148
9 18 0	46½	173	34½	86		148
9 19 30	47	174	34	86		148
9 20 30	47	174	34	86		147
9 22 30	47	174	34	86		147
9 23 30	47	173	34	86		146
9 24 30	47	173	34½	86		145½

Experiment VI. made January 11, 1782.

Time *per* watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.	Mer. ther. A in air.
h. ′ ″						
9 25 30	47	173	34½	86	Sunk inſtantaneouſly.	438
9 26 30	47	173	34½	86		436
9 27 45	47	173	34½	86		436
9 28 30	46	174	34½	86		435
9 29 15	46	173	34½	86		433
9 30 0	46	173	34½	86		432½
9 31 0	46	172	34½	86		432
9 31 45	46	172	34½	86		430
9 33 40	47	174	34½	86		430
9 34 30	47	174	34½	86	I ſuſpect this variation to have been occaſioned by different perſons reading off the numbers.	430
9 35 30	47	173	34½	86		428
9 36 0	47	173	34½	86		426
9 37 0	47	173	34½	86		426
9 38 0	47	173	34½	86		426
9 39 0	47	173	34½	86		425
9 40 0	47	173	34½	86		424
9 41 0	47	173	34½	86		424
9 42 0	47	173	34½	86		423
9 43 0	47	173	34½	86		422
9 45 0	47	172	34	86		420
9 48 0	47	172	34	86	Put in apparatus (F) at +40°.	412
9 49 0	47	172	34	86		412
9 50 0	47	172	34	86	Took out apparatus (F) ſolid −40°.	412
9 51 0	47	172	34	86	Put in apparatus (G) fell directly to −210°.	410
2 53 0	47	172	34	86	Took out ditto, ſolid at 210°.	405
9 55 0	47	172	34	86		402
9 56 0	47	172	34	86		401
9 57 0	47	172	34½	86		400
9 58 0	47	172	34½	86		398
9 59 0	47	172	34½	86		396
10 0 0	47	172	34½	86		394
10 1 0	47	172	34½	86		392
10 2 0	47	172	34½	86		390
10 3 0	47	172	34½	86		388
10 4 0	47	172	34½	86		386

Experiment

Experiment VI. made January 11, 1782.

Time per Watch.	Standard fpirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.	Mercurial B in air.
h. ′ ″						
10 5 0	47	172	34	86		384
10 6 0	47	172	34	86	Apparatus (G) in the bulb, in open air, fince 9 h. 53′.	382
10 9 0	47	172	34	86		372
10 10 0	47	172	34	86		370
10 11 0	47	172	34	86		366
10 13 0	47	172	34	86		362
10 14 0	47	172	34	86		360
10 15 0	47	172	34	86		358
10 16 0	47	172	34	86		356
10 17 0	47	172	34	86		353
10 18 0	47	172	34	86		351
10 19 0	47	172	34	86		348
10 20 0	47	172	33½	86		346
10 21 0	47	172	33½	86		342
10 22 0	47	172	33½	86		340
10 23 0	47	172	33½	86		338
10 24 0	47	171	33½	86		334
10 25 0	47	171	33½	86		332
10 26 0	47	171	33½	86		330
10 27 0	47	171	34	86		328
10 28 0	47	171	34	86		324
10 29 0	47	171	34	86		320
10 30 0	47	171	34	86		316
10 31 0	47	171	34	86		314
10 32 0	47	171	34	86		300
10 32 30	47	171	34	86		298
10 33 0	47	171	34	86		286
10 33 30	47	171	34	86		274
10 34 0	46	171	34	86		272
10 35 0	46	171	34	86		268
10 36 0	46	171	34	86		264
10 37 0	46	171	34	86		262
10 38 0	46	171	34	86		260
10 39 0	46	171	34	86		258
10 40 0	46	171	33½	86		254
10 41 0	46	171	33	86		246

Experiment

Experiment VI. made January 11, 1782.

Time *per* Watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.	A in air. Mer. ther.
h. ′ ″						
10 42 0	46	171	32½	86		244
10 43 0	46	170	32½	86		238
10 44 0	46	170	32½	86		234
10 45 0	46	170	32½	86		230
10 46 0	46	170	32½	86		228
10 47 0	46	170	32¼	86	External part of quickſilver in the apparatus (F) fluid, the center a globular ſolid. In the open air an hour.	224
10 49 0	46	170	32½	86		214
10 50 0	46	170	32½	86		218
10 51 0	46	170	32½	86		208
10 52 0	46	170	32	86		206
10 53 0	46	170	32	86		200
10 54 0	45	170	32	86		196
10 55 0	45	170	32	86		192
10 56 0	45	170	32	86		190
10 57 0	45	170	32	86		186
10 58 0	45	170	31½	86		176
10 59 0	45	170	31½	86		166
11 0 0	45	170	31½	86	A portion of quickſilver in apparatus (F) ſtill frozen.	160
11 5 0	44	170	31	85		134
11 6 0	44	170	31	85		128
11 7 0	44	170	31	85		126
11 8 0	44	170	31½	86		124
11 9 0	44	170	31½	86		116
11 10 0	44	170	31½	86		116
11 11 0	44	170	31½	86		110
11 12 0	44	170	31½	86		100
11 13 0	44	170	31½	86		96
11 14 0	44	170	31½	86		92
11 15 0	44	170	31½	86		84
11 16 0	44	170	31½	86		82
11 17 0	44	170	31	85		78
11 18 0	44	170	30¾	85		68
11 19 0	44	166	30¼	85		60
11 20 0	44	167	30	85		54

Experiment VI. made January 11, 1782.

Time *per* Watch.	Standard Spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and observations.	Mer. ther. A in air.
h. ′ ″						
11 22 0	43	167	30	85		48
11 23 0	43	167	30	85		44
11 25 0	43	167	30	85		42
11 26 0	43	167	30	85		40
11 27 0	43	167	30	85		40
11 28 0	43	167	30	85		40
11 29 0	43	167	30	85		40
11 30 0	43	167	30	85		40
11 31 0	43	167	30	85		40
11 32 0	43	167	30	85		38
11 33 0	43	167	29½	85		36
11 34 0	43	167	29½	85		36
11 35 0	43	167	29½	85		36
11 36 0	43	167	29½	85		36
11 37 0	43	167	29½	85		36
11 38 0	43	167	29½	85		36
11 39 0	43	167	29½	85		36
11 40 0	43	167	29½	85		36
11 42 0	43	167	29½	85		36
11 44 0	43	167	29½	85		36
11 46 0	43	167	29½	85		34
11 48 0	42½	166	29	85		34
11 49 0	42	165	28½	434	The quickſilver in thermometer (B) ſunk in an inſtant.	34
11 51 0	42	165	28½	432		34
11 52 0	42	165	28½	432		34
11 53 0	42	165	28½	432		34
11 53 30	42	165	28	430		34
11 54 0	42	166	28	427		
11 55 0	42	315	28	425	The quickſilver in thermometer (C) ſubſided all at once.	
11 56 0	41	360	28	422		
11 57 0	41	360	28	420		
11 58 0	40½	358	28	417		
11 58 30	40½	355	27½	415		
11 59 0	40	352	27½	413		

Experiment

Experiment VI. made January 11, 1782.

Time per Watch.	Standard Spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.
h. ′ ″					
12 0 0	40	350	27½	408	
12 1 0	40	345	27¼	404	
12 2 0	40	336	27	398	
12 3 0	40	330	27	394	
12 4 0	40	323	27	390	
12 5 0	40	315	27	384	
12 6 0	40	309	27	379	
12 7 0	40	300	27	374	
12 8 0	40	294	27	368	
12 9 0	40	285	27	362	
12 10 0	40	275	27	355	
12 12 0	40	257	27	343	
12 13 0	40	245	27	335	
12 14 0	40	235	26½	329	
12 15 0	40	222	26½	320	Made a fresh mixture.
12 16 0	—	—	—	—	Put the instruments into the fresh mixture.
12 16 30	40½	270	33	340	
12 17 0	42	335	33	340	
12 17 30	42	365	33	340	
12 18 0	42	370	33½	340	
12 18 30	42	372	33½	340	
12 19 0	42½	372	33½	340	
12 20 0	43	372	33½	340	
12 21 0	43	372	33½	340	
12 22 0	43	372	33½	340	
12 24 0	43	374	33½	340	
12 26 0	43	374	33	340	
12 28 0	43	374	33	340	
12 32 0	43	374	33	340	
12 37 0	43	374	33	340	
12 42 0	43	374	32	340	
12 45 0	43	374	32	340	
12 54 0	43	374	32	340	
12 59 0	43	374	32	340	
1 0 0	43	374	32	340	

Experiment VI. made January 11, 1782.

Time *per* Watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.
h. ′ ″					
1 5 0	42	373	30	340	
1 10 0	42	371	29	340	
1 14 0	41½	370	29	340	
1 19 0	41	370	28½	340	
1 21 0	41	370	29	340	Added snow, the mixture growing thin.
1 23 0	41	370	29	340	
1 26 0	41	370	28½	340	
1 30 0	40½	370	28	340	
1 35 0	40	370	28	340	
1 40 0	40	370	28	340	
1 45 0	40	360	27	340	
1 46 30	40	—	27	400	
1 46 45	40	—	27	410	
1 47 0	40	—	27	438	
1 49 0	39	335	26½	433	
1 50 0	39	322	26	428	
1 51 0	38	312	26	423	
1 54 0	38	291	26½	413	
1 55 0	38	280	26½	408	
1 56 0	38	273	26½	403	
1 57 0	38	266	26½	398	
1 58 0	38	259	26½	394	
1 59 0	38	253	26½	388	
2 0 0	38	241	26½	382	
2 1 0	38	232	26½	376	
2 2 0	38	225	26½	371	
2 3 0	38	212	26½	363	
2 4 0	38	202	26½	357	
2 5 0	38	193	26½	350	
2 6 0	37½	180	26½	341	
2 7 0	37½	167	26½	332	
2 8 0	37½	158	26	328	
2 9 0	38	145	26	320	
2 10 0	38	132	26	310	
2 11 0	38	123	26	304	
2 12 0	38	105	26	300	

Experiment

Experiment VI. made January 11, 1782.

Time per Watch.	Standard Spirit H.	Spirit therm. C. Mercurial	Spirit therm. D.	Mercurial therm. B.	Remarks and Occurrences.
h. ′ ″					
2 13 0	38	80	26	282	
2 14 0	38	55	26	276	
2 15 0	38	37	26	270	
2 16 0	38	45	26	262	
2 17 0	38	35	26	252	
2 18 0	38	35	26	250	
2 19 0	37½	34	26	244	Took out all the inſtruments.

This ſingular experiment, though it did not anſwer the intention for which it was principally deſigned, yet afforded many ſtriking phænomena which I ſhall mention in the courſe of theſe remarks. After a cold night, the quickſilver in the thermometer was at 44° below o at ſeven o'clock in the morning: thinking this great degree of cold was the moſt favourable opportunity of obſerving how low it was poſſible to make the quickſilver deſcend in the tube of the thermometers, I reſolved to embrace it, and at the ſame time to obſerve the concurrent degrees with a ſpirit thermometer; but as thoſe ſent out to me in 1781 (D and E) differed ſo much from the thermometers of quickſilver, I reſolved to make uſe of another ſpirit thermometer made by NAIRNE and BLOUNT, and which was alſo furniſhed me by the Royal Society in 1774. With this inſtrument, which I call the ſtandard, and marked with the letter H, I have made obſervations eight years, and found it agree very well with others made of quickſilver; and the more readily to diſcover the variation of (D), I employed it alſo in the ſame experiment; but before I began the following obſervations

were taken, the inſtruments all expoſed to the open air, where they are continually kept. The thermometers are marked from (A) to (H), and the obſervations are regularly in that order.

h. ′	A	B	C	D	E	F	G	H
7 45	44½	45	41	28	29	40	40	46
7 50	46	64	124	30	32	42	41	46
7 55	–	–	60	–	–	–	–	–
7 57	44	–	–	–	–	–	–	–

It is obſervable, that neither the quickſilver which was in the cylinders affixed to (F) and (G), nor the other quickſilver which I conſtantly kept in the ſame place, ſome in a ſaucer, ſome in a gallipot, and ſome in a phial, ſhewed the leaſt appearance of congelation. Being engaged in preparing for the enſuing experiment, I did not remark either the great deſcent or aſcent of the quickſilver in (C), which muſt have been very ſudden, as my remarks are only five minutes aſunder.

It may be neceſſary to mention, that the thermometer (H) was mounted on a ſcale the whole length (as uſual for meteorological obſervations), and (C) was armed with elaſtic gum from the bulb to about half or three-quarters of an inch above the ſurface of the freezing mixture.

The ſmall deſcent of the quickſilver in (C), and the little effect produced by moving it into a ſecond mixture, made me at firſt apprehend the inſtrument was damaged; I did not, however, take it out, but took another thermometer (A), and put it alſo in the mixture; but I find it was ſtationary at a higher degree than (C): I therefore exchanged (A) for the mercurial thermometer (B), which to my great ſurprize was ſtationary at 86°, nor could it be got lower until the cold of the mixture diminiſhing it fell

at

at once to 434°, and a few minutes afterwards (C) fell to 360°. Imagining that a new mixture would now bring it very low, I made another, but in the mean time the inſtruments had riſen greatly, and after ſtanding in the freſh mixture (C) ſunk to 374°, and (B) to 438°. I ſhould have mentioned, that theſe mixtures were double in quantity to thoſe uſed in the former experiments; inſtead of glaſs tumblers, they were made in pint baſons.

I obſerved alſo, that the mixtures ſeemed to grow thin ſooner than common; for I always made them of the conſiſtence of pap. I added ſnow at times, to thicken it, but found it had very little effect, but rather decreaſed the cold. It is with great diffidence I offer it as my opinion, that the temperature of the air was too cold, and that the quickſilver being nearly in a ſtate of congelation before plunged into the mixture, was inſtantly frozen on putting the inſtruments into them; and as the quickſilver in the tubes muſt have been of the ſame temperature with that in the bulbs of the thermometers before the experiment, I ſhould imagine, that when the quickſilver in the bulb was frozen ſolid, it communicated an addition of cold to that in the tube, and froze it alſo, which prevented its ſubſiding as uſual; for in other caſes, the contraction of the quickſilver, when ſolid in the bulb, was the cauſe of the quickſilver ſubſiding in the tube; but then the latter was fluid, for the circumambient air was warmer than the degree at which quickſilver freezes, and the increaſed cold was applied only to the bulb. The obſervations made before the experiment began, as related in the beginning of theſe remarks, ſhew the quickſilver in the thermometer was congealing, and that (A) and (C) were actually frozen.

When

When I removed the thermometer (A) out of the mixture at 9 h. 5′, I hung it up in the air, and have noted down, in a ſeparate column on the right-hand ſide of the page, its appearances correſponding to the times put down on the other ſide of the page. It is remarkable, that (A) and (C) have each an air-bubble blown at the top; but the thermometer (B) had none.

Whilſt the inſtruments were ſtationary in the foregoing experiment, I put the apparatus (F) and (G) ſeverally into the mixture with the others; the conſequence was, that in two minutes the quickſilver in the cylinder was frozen ſolid; but as there was a difference in the effect I ſhall be more particular. At 9 h. 48′ put in apparatus (F), when it ſtood in the air at 40° or 41° below 0; and at 9 h. 50′ took it out frozen ſolid, and the incloſed thermometer pointing ſtill at 40° or 41°. I then hung it up in the open air, and looked at it only now and then. At 10 h. 47′ (after being expoſed to the air near an hour), I found only a ſmall quantity of the ſurface of the quickſilver was fluid, the reſt was a frozen globe reſembling a ball of poliſhed ſilver; the thermometer incloſed was ſtill at 40°. At 11 h. 4′ I obſerved a ſegment of a globe of ſolid quickſilver; in the inſide was a concavity made, I ſuppoſed, by the bulb of the thermometer. The thermometer was ſtill at 40°, which undoubtedly is the freezing point of quickſilver, as in this inſtance part of it was frozen, and part ſolid. I withdrew the thermometer, poured out the fluid quickſilver, and returned the thermometer into the cylinder, ſhortly after which it was at 37°, and the frozen ſegment was then fluid.

The apparatus (G) was hanging in the open air at 40°, and put into the ſame freezing mixture at 9 h. 51′, on which it ſunk inſtantly to 210°, at which degree it was ſtationary at

9 h.

9h. 53′, when it was taken out of the mixture perfectly ſolid. At 10h 6′ I ſaw it had ſubſided into the bulb (I mean the quickſilver in the incloſed thermometer) which was the laſt time I particularly noticed it. It may be neceſſary to mention, that finding the quickſilver in the encloſed thermometer ſink inſtantaneouſly as ſoon as the apparatus was put into the freezing mixture, I took it out immediately, to view it, and replaced it in a few ſeconds of time. I found the quickſilver was not yet ſolid, but was in frozen pieces of irregular ſhapes, reſembling ice that had been broken to pieces by concuſſion in a pail of water, but with this remarkable difference, that as ice ſwims on the water, the frozen quickſilver ſubſided in fluid quickſilver, and the ſegment of ice, mentioned a little before to be found in the thermometer (F) was alſo at the bottom of the cylinder, and remained there after decanting the liquid quickſilver from it. Hence we may conclude, that cold increaſes the gravity of quickſilver, as indeed muſt be the caſe, ſince it is certain it occupies leſs ſpace in a ſolid than in a fluid ſtate.

Experiment VII. made January 22, 1782.

Time *per* Watch.	Standard ſpirit H.	Mercurial therm. C.	Spirit therm. I.	Remarks and Occurrences.
h. ′ ″				
8 42 0	—	—	—	Making the freſh mixture.
8 45 0	—	—	—	Put in the inſtruments.
8 45 30	38	65	31½	
8 46 0	41	105	33	
8 46 15	42	130	33	
8 46 30	43	155	33½	
8 46 45	44	183	33½	
8 47 15	44	207	33½	
8 47 45	44	235	34	
8 48 0	44½	235	34	
8 48 30	45	235	34	
8 49 0	45	235	34	
8 50 0	45	235	34	
8 51 0	45	235	34	
8 52 0	46	237	34	
8 53 0	46	238	34	
8 54 0	46	238	34	
8 55 0	46	238	34	
8 56 0	46	237	34	
8 57 0	46	237	34	
8 58 0	46	236	34	
9 0 0	46	236	34	Making a freſh mixture.
9 4 0	45½	236	33	
9 5 0	—	—	—	Removed the inſtruments into the new mixture.
9 5 15	44	235	32	
9 5 45	44	237	32	
9 6 0	44½	237	31	
9 8 0	—	—	—	Put in apparatus (G).
9 9 0	—	—	—	(G) ſunk into the bulb, quickſilver in the cylinder fluid.
9 11 0	44½	237	30½	
9 13 0	44½	237	29½	
9 15 0	44½	237	29½	
9 16 0	44½	237	29½	
9 17 0	44	237	29½	
9 18 0	44	237	29½	
9 19 0	43½	237	29½	

Experiment VII. made January 22, 1782.

Time *per* Watch.	Standard spirit H.	Mercurial therm. C.	Spirit therm. D.	Remarks and Occurrences.
h. ′ ″				
9 20 0	43½	237	29½	
9 21 0	43½	237	29	
9 22 0	33	236	29	Examined (G) remains as the laſt time.
9 23 0	42½	236	29	
9 24 0	42½	235	29	
9 25 0	42½	235	29	(G) remains ſtill the ſame.
9 26 0	42½	235	29	
9 27 0	42	235	29	
9 28 0	42	234½	29	
9 29 0	42	234	29	
9 30 0	42	234	29	
9 31 0	41½	234	29	
9 32 0	41	234	29	
9 33 0	41	234	29	
9 34 0	41	234	28½	
9 35 0	41	234	28½	
9 36 0	41	234	28½	
9 37 0	40½	234	28½	
9 38 0	40½	234	28	
9 39 0	40½	234	27½	
9 40 0	40	234	27	
9 41 0	40	234	27	
9 42 0	40	234	26½	
9 43 0	40	233	26½	
9 44 0	40	232	26½	
9 45 0	40	232	26½	
9 46 0	39½	232	26½	
9 47 0	39½	232	26½	
9 48 0	39	232	26	
9 49 0	39	231½	26	(G) remains in the bulb, quick. in the cylinder fluid.
9 50 0	39	231½	26	
9 50 30	39½	231	26	
9 51 0	39	232	26	
9 52 0	39	650	26	
9 52 20	—	850	—	
9 52 30	—	1050	—	
9 52 40	—	1120	—	

Experiment VII. made January 22, 1782.

Time per Watch.	Standard ſpirit. H.	Mercurial therm. C.	Spirit therm. D.	Remarks and Occurrences.
h. ′ ″				
9 52 50	—	1300	—	
9 53 0	—	1350	—	
9 53 10	—	1360	—	
9 53 30	38	1361	26	
9 54 0	38	1361	26	Apparatus (G) as before; took it out entirely.
9 56 0	38	1362	25½	
9 57 0	38	1362	25½	Made a freſh mixture.
9 58 30	38	1305	31	Put the inſtruments into the new mixture.
9 59 30	39	1305	32	
10 0 0	40	1305	32	
10 0 30	40	1305	32	
10 1 0	40	1306	32	
10 2 0	40	1306	32	
10 3 0	40	1306	32	
10 4 0	40	1307	32	
10 5 0	40	1306	32	
10 6 0	40	1306	31½	
10 7 0	40	1306	31½	
10 8 0	40	1306	31½	
10 9 0	40	1307	31	
10 10 0	40	1307	31	
10 11 0	40	1307	31	
10 12 0	39½	1307	30½	
10 13 0	39½	1307	30½	
10 14 0	39	1306	30	
10 15 0	39	1306	30	Found (C) has loſt its bulb in the former mixture.

Remarks on the ſeventh experiment.

From the ſixth experiment I was induced to think, that the nearer the temperature of the atmoſphere approached to the freezing point of quickſilver, ſo that a great degree of cold might be communicated to the bulb of a thermometer and yet the quickſilver in the tube remain fluid, would be the pro-

perest time for ascertaining in this manner to what degree quicksilver will contract by the application of cold. With this view this seventh experiment was made: the several thermometers from A to H were as follows, before I began, A 38, B 36, C 33, D 24, E 241, F 33, G 33, H 37. Those used in the experiment were C, D and H. The first was to shew the descent of the quicksilver; and the two last, which were spirit thermometers, were employed to shew the corresponding contractions of the two substances, quicksilver and alcohol. After above an hour's attendance on them, I was highly pleased to see the quicksilver fall to 1367° below the cypher, especially as I supposed, by changing the mixture for a fresh one, I should get it much lower still. I made another accordingly, and removed the instruments into it. The quicksilver rose, as was common in changing the mixtures; but after waiting a considerable time, without its descending again, I recollected Professor BRAUN mentioning that his thermometers were always broken when below 600°. This made me examine mine, and I found the bulb was broken and fallen off; and on a diligent search in the mixture, I could not find either quicksilver or the pieces of glass; I therefore conclude it had dropped off into the other mixture, which unluckily I had thrown away the moment before, having occasion to use the bason in decanting the present mixture: I have no doubt but it broke at the time the quicksilver fell so rapidly. During the course of this experiment I put the apparatus (G) into the freezing mixture; in a minute's time the quicksilver in the inclosed thermometer had subsided into the bulb, and remained so during the time it continued immersed in the freezing mixture, which was about three quarters of an hour; but though the thermometer, which made part of the apparatus, shewed so great a degree of cold,

yet the quickſilver in the cylinder was never frozen; and indeed the ſpirit thermometers, ſuſpended in the mixture, ſeemed to indicate, that there was not ſufficient cold to freeze quickſilver, except at the beginning; for I obſerve, it is not effected at 40°, without continuing ſome time at that degree, as appears very clearly from the third experiment.

Experiment VIII. made December 21, 1781.

Time *per* Watch.	Mercurial thermom.	Apparatus.	Spirit Thermom.	Remarks and Occurrences.
h. ′ ″				
10 10 0	—	—	—	Made the freezing mixture.
10 12 0	—	—	—	Put in the gallipot of quickſilver.
10 13 0	—	—	—	Put thermometer (B) into the quickſilver.
10 13 30	—	35	—	
10 13 35	—	36	—	
10 13 40	—	37	—	
10 14 0	—	37	—	Put thermometer (A) into the freezing mixture.
10 14 30	30	38	—	
10 14 40	40	38	—	
10 15 0	42	38	—	
10 15 15	43	39	—	
10 15 30	44	39	—	
10 16 0	48	39	—	
10 16 15	56	39	—	
10 16 30	64	39	—	
10 16 45	70	39	—	
10 17 0	76	39	—	
10 17 15	81	39	—	
10 17 30	87	39	—	
10 17 45	92	39	—	
10 18 0	97	39	—	
10 18 15	102	39	—	
10 18 30	108	39	—	
10 18 45	113	39	—	
10 19 0	118	39	—	
10 19 15	122	39	—	
10 19 30	126	39	—	
10 19 45	130	39	—	

Experiment

Experiment VIII. made December 21, 1781.

Time *per* Watch.	Mercurial thermom.	Apparatus.	Spirit thermom.	Remarks and Occurrences.
h. ′ ″				
10 20 0	136	39	—	
10 20 15	139	39	—	
10 20 30	143	39	—	
10 20 45	147	39	—	
10 21 0	151	39	—	
10 21 15	154	39	—	
10 21 30	159	39	—	
10 21 45	163	39	—	
10 22 0	166	39	—	
10 22 15	169	39	—	
10 22 30	173	39	—	
10 22 45	175	39	—	
10 23 0	178	39	—	
10 23 15	182	39	—	
10 23 30	185	39	—	
10 23 45	187	39	—	
10 24 0	189	39	—	
10 24 15	192	39	—	
10 24 45	197	39	—	
10 25 0	199	39	—	
10 25 15	201	39	—	
10 25 45	188	39	—	Thermometer (A) slipped into the gallipot containing the quickfilver, by accident; replaced it.
10 27 15	200	39	—	
10 30 45	127	39	—	
10 31 0	122	39	—	
10 32 0	—	—	—	Put a ſpirit thermometer into the mixture.
10 32 30	96	39	27	
10 33 0	86	39	27	
10 33 30	80	39	27½	
10 33 45	72	40	27½	
10 34 30	58	40	27	Made another mixture.
10 38 45	38	40	27	Took out the inſtruments.
10 40 0	—	—	—	Changed the mixture.
10 40 30	59	41	—	
10 40 45	63	41	—	
10 41 0	67	41½	—	

Experiment VIII. made December 21, 1781.

Time *per* Watch.	Mercurial thermom.	Apparatus.	Spirit thermom.	Remarks and Occurrences.
h. ′ ″				
10 41 10	70	41½	—	Put in the spirit thermometer.
10 41 30	78	41½	29	
10 42 0	84	41½	30½	
10 42 15	88	41½	31	
10 42 30	91	41½	31	
10 43 0	95	41	31	
10 43 15	99	41	31	
10 43 30	100	41	31	
10 43 45	101	41	31	
10 44 0	101	41	31	
10 44 30	101	41	31	
10 45 15	101	40½	31	
10 46 0	101	40	30¾	
10 47 0	101	40	30½	
10 47 30	101	40	30½	
10 48 0	101	40	30¼	
10 48 30	101	40	30	
10 49 0	101	40	30	
10 49 30	101	40	29¾	
10 50 0	101	40	29¾	
10 50 30	101	40	29¾	
10 51 0	101	39½	29¾	
10 51 30	101	39½	29½	
10 52 0	101	39½	29	
10 52 30	101	39½	29	
10 53 0	101	39	29	
10 54 0	113	39	29	The descent in the mercurial therm. was sudden.
10 54 30	112	39	29	
10 54 45	111	39	28¾	
10 55 0	110	39	28¾	
10 55 30	108	39	28¼	
10 55 45	106	38¾	28¼	
10 56 0	104	38¾	28¼	
10 56 30	102	38½	28¼	
10 57 0	99	38½	28¼	
10 57 30	96	38½	28	
10 58 0	93	38½	27½	

Experiment VIII. made December 21, 1781.

Time per watch.	Mercurial thermom.	Appara-tus.	Spirit thermom.	Remarks and Occurrences.
h. ′ ″				
10 58 30	89	38¼	27½	
10 59 0	85	38	27½	
10 59 30	82	38	27½	
11 0 0	78	38	27½	Stirred the mixture.
11 0 30	75	38	27½	
11 1 0	70	38	27½	
11 1 30	66	38	27½	
11 2 0	64	38	27½	
11 2 30	60	38	27½	Stirred the mixture.
11 3 0	55	38	27½	
11 3 30	51	37¾	27	
11 4 0	45	37¼	27	
11 4 30	44	37	27	

This eighth experiment was made with a view to try, whether quickſilver would freeze whilſt in contact with the freezing mixture. For this purpoſe I did not uſe the apparatus employed in the other examples, but ſubſtituted another, by taking a gallipot made of flint ſtone (as being thinner than the common ſort) of about an ounce meaſure, and filled it half full of quickſilver, into which I inſerted the mercurial thermometer (B) and employed the other mercurial thermometer (A) as an index, as before. I hoped by this means to determine exactly when the quickſilver was congealed, as I had free acceſs to it at all times, which was not the caſe when incloſed in the cylindrical glaſs, the worſted wound round the tube of the ivory thermometer to exclude the air, equally excluding any inſtrument from being introduced to touch the quickſilver. I made a kind of ſkewer, with a flat blunt point, of dried cedar wood

wood for lightneſs, which I found would remain in the gelatinous freezing mixture at any depth I choſe; but when inſerted into the quickſilver contained in the gallipot, the great diſproportion of gravity made it rebound upwards, and by the touch I could eaſily perceive, by the reſiſtance it met with, whether it proceeded from quickſilver in a fluid or congealed ſtate. The event did not anſwer my wiſhes, for I could not find that the quickſilver was frozen in the leaſt during the trial. Indeed the temperature of the air was not favourable, being under 20° below the cypher. The large quantity too of the quickſilver in the gallipot, as well as the thickneſs of that veſſel, might both of them contribute to render the operation unſucceſsful; yet, as the apparatus thermometer ſhewed the ſame degree (−40) as when quickſilver froze in the glaſs cylinder, I am of opinion it would congeal by this ſimple method in very cold weather, and a long continued application of a proper degree of cold by the mixtures.

Experiment IX. made February 22, 1782.

Whilſt I was attending on the preceding experiment (the 5th) and had removed the inſtruments into a ſecond mixture, the former one by this means being unemployed, I put into it a gallipot (the ſame I uſed in the eighth experiment) with about three quarters of a pound of quickſilver, and let it remain immerſed in the mixture a conſiderable time (I ſuppoſe near half an hour), and finding, by touching with a quill, that part of it was congealed, I drew the gallipot out, it being previouſly ſlung with a ſtring, and decanted off the ſuperincumbent mixture and fluid quickſilver; the remainder, about two-thirds of the whole quantity, remained ſolid in the gallipot;

pot; the internal ſurface remained every where very rough and white, ſhining like an old ſilver ſpoon long in uſe and having loſt its poliſh. Part of it became fluid in a few minutes; and imagining it afforded a fine opportunity of confirming what had before appeared to be the freezing point of quickſilver, I put a mercurial thermometer (F) which then ſtood at 34°, into the part of the quickſilver in the gallipot, which was juſt thawed, and it ſubſided directly to −40°, and became ſtationary. I repeated the ſame with another inſtrument, and the conſequence was the ſame. I then tried the ſpirit thermometer (D) which became ſtationary at 28°½; and another ſpirit thermometer (E) which I took out of the freezing mixture, where it was at 35°, and it roſe to 30°; and by comparing the ſpirit thermometers with mercurial ones, and alſo with another ſpirit thermometer (H) it appears, that 29° on the former is about equal to 40° on the ſcale of both the latter. By the time theſe obſervations were taken, the frozen lump was looſened in the gallipot: I turned it out, and beat it with an hammer; it yielded a dead ſound and flattened, but its coheſion was very weak; for, inſtead of expanding into a thin plate, as in other inſtances when frozen in the bulb of a thermometer, it crumbled to pieces, and had not that poliſh, which I had before conſtantly obſerved. I attributed theſe circumſtances to the effect of the ſpirit of nitre on the quickſilver. It thawed very ſoon after its parts were disjoined by the ſtroke of the hammer.

Experiment X. made January 26, 1782.

Quickſilver frozen by the natural cold in Hudſon's Bay.

The ſubject of this curious phænomenon was quickſilver put into a common two-ounce phial, and corked. The phial was about a third part full, and had been conſtantly ſtanding by the thermometer for a month paſt. At eight o'clock this morning I obſerved it was frozen rather more than a quarter of an inch thick round the ſides and bottom of the phial, the middle part continuing fluid. As this was a certain method to find the point of congelation, I introduced the mercurial thermometer (F) and the ſpirit thermometer (D) into the fluid part, after breaking off the top of the phial, and they roſe directly and became ſtationary; the former at 40° or 40°¼, the latter at 29°¾, both below the cypher. Having taken theſe out, I put in two others, (G) and (E); the former became ſtationary at 40°, the latter at 30°. I then decanted the fluid quickſilver, to examine the internal ſurface of the frozen quickſilver, which proved very uneven, with many radii going acroſs; ſome of theſe reſembled pins with heads. Urgent buſineſs called me away an hour. On my return I found a ſmall portion only had liquified in my abſence. I then broke the phial entirely, and with a hammer repeatedly ſtruck the quickſilver. It beat out flat, yielded a deadiſh ſound, and became fluid in leſs than a minute afterwards. I ſhould have mentioned, that I brought the thermometer (F) into a room, where it roſe to 55° above the cypher, and then let it cool again in the open air, before I put it into the frozen quickſilver. My reaſon was, for fear the quickſilver in the thermometer ſhould be frozen ſo as to render

the obſervation uncertain; but I did not obſerve it differed any thing of conſequence from (G) which was not taken in, but put directly into the phial. By the comparative obſervations of the ſeveral thermometers it appears, that 30° on the ſcale of the ſpirit thermometers (D) and (E) is about equal to 40° or 41° on my ſtandard ſpirit thermometer (H). The following was the ſtate of the inſtruments that morning,

	A.	B.	D.	E.	F.	G.	H.
At eight	−103	−80	33½	33	42½	42	46
At nine	−323	−444	−29	−29½	−40	−40	−44
At noon	34	32	21	21½	30	29½	34

It may be worth remarking, that the quickſilver in the thermometer (B) which had been very near 500°, and was then at 444°, very readily run up and down the tube by elevating either end of the inſtrument.

EXPLA-

EXPLANATION OF PLATE VII.

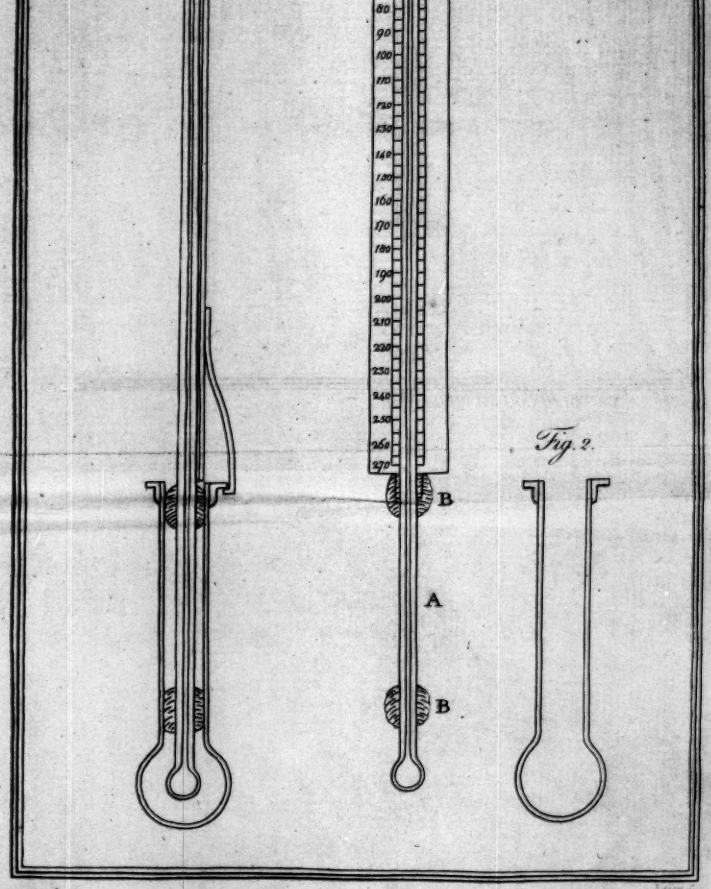
Fig. 1. The thermometer ſeen in front.

A. The ſtem and bulb reaching below the ſcale.

B. B. Worſted wrapped round the ſtem, in order to keep it ſteady in the cylinder, and prevent the acceſs of air.

Fig. 2. The cylinder, ſwelled at bottom, to hold the quick-ſilver to be frozen.

Fig. 3. A ſection of the whole apparatus when put together, ſhewing in what manner the thermometer is inſerted and retained in the cylinder. This ſection is perpendicular to the ſcale of the thermometer.


80
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220
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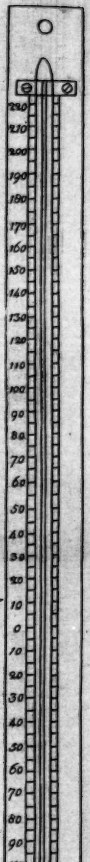
260
270
B
A
B

Fig. 2.

Fig. 3.

Fig. 1.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180
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140
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90

XX. *Observations on Mr.* Hutchins's *Experiments for determining the Degree of Cold at which Quickſilver freezes.* By Henry Cavendiſh, *Eſq. F. R. S.*

Read May 1, 1783.

THE deſign of the following paper is to explain ſome particulars in the apparatus ſent by me to Mr. HUTCHINS, the intention of which does not readily appear; and alſo to endeavour to ſhew the cauſe of ſome phenomena which occurred in his experiments; and point out the conſequences to be drawn from them.

This apparatus was intended to determine the preciſe degree of cold at which quickſilver freezes: it conſiſted of a ſmall mercurial thermometer, the bulb of which reached about 2½ inches below the ſcale, and was incloſed in a glaſs cylinder ſwelled at bottom into a ball, which, when uſed, was filled with quickſilver, ſo that the bulb of the thermometer was intirely ſurrounded with it. If this cylinder is immerſed in a freezing mixture till great part of the quickſilver in it is frozen, it is evident, that the degree ſhewn at that time by the incloſed thermometer is the preciſe point at which mercury freezes; for as in this caſe the ball of the thermometer muſt be ſurrounded for ſome time with quickſilver, part of which is actually frozen, it ſeems impoſſible, that the thermometer ſhould be ſenſibly above that point; and while any of the quickſilver in the cylinder remains fluid, it is impoſſible that it ſhould ſink ſenſibly

below it. The ball of the thermometer was kept conſtantly in the middle of the ſwelled part of the cylinder, without danger of ever touching the ſides, by means of ſome worſted wound round the tube. This worſted alſo ſerved to prevent the acceſs of the air to the quickſilver in the cylinder, which, if not prevented, would have made it more difficult to have communicated a ſufficient degree of cold. The diameter of the bulb of the thermometer was rather leſs than one-fourth of an inch, that of the ſwelled part of the cylinder was two-thirds, ſo that there was no where a much leſs thickneſs of quickſilver between the ball and cylinder than one-ſixth of an inch. The bulb of the thermometer was purpoſely made as ſmall as it conveniently could, in order to leave a ſufficient ſpace between it and the cylinder, without making the ſwelled part thereof larger than neceſſary, which would have cauſed more difficulty in freezing the quickſilver in it. Two of theſe inſtruments were ſent for fear of accidents.

One of the moſt ſtriking circumſtances in the experiments which have been made for freezing mercury, is the exceſſively low degree to which the thermometers ſunk, and which, if it had proceeded, as was commonly ſuppoſed from the freezing mixture having actually produced ſuch a degree of cold, would have been really aſtoniſhing. The experiments, however, made at Peterſburg afforded the utmoſt reaſon to ſuppoſe, and Mr. HUTCHINS's laſt experiments have put beyond a poſſibility of doubt, that quickſilver contracts in the act of freezing, or in other words, that it takes up leſs room in a ſolid than in a fluid ſtate; and that the very low degree to which the thermometers ſunk was owing to this contraction, and not to the intenſity of the cold produced: for example, in one of Mr. HUTCHINS's experiments a mercurial thermometer, placed in the

the freezing mixture, ſunk to 450° below nothing, though the cold of the mixture was never more than −46; ſo that the quickſilver was contracted not leſs than 404° by the action of freezing.

If a glaſs of water, with a thermometer in it, is expoſed to the cold, the thermometer will remain perfectly ſtationary from the time the water begins to freeze till it is intirely congealed, and will then begin to ſink again. In like manner, if a thermometer is dipped into melted tin or lead, it will remain perfectly ſtationary, as I know by experience, from the time the metal begins to harden round the edges of the pot till it is all become ſolid, when it will again begin to deſcend; and there was no reaſon to doubt that the ſame thing would obtain in quickſilver.

From what has been juſt ſaid it was concluded, that if this apparatus was put into a freezing mixture of a ſufficient coldneſs, the thermometer would immediately ſink till the quickſilver in the cylinder began to freeze, and would then continue ſtationary, ſuppoſing the mixture ſtill to keep cold enough, till it was intirely congealed. This ſtationary height of the thermometer is the point at which mercury freezes, though in order to make the experiment convincing, it was neceſſary to continue the proceſs till ſo much of the quickſilver in the cylinder was frozen as to put the fact out of doubt.

If the experiment had been tried with no further precautions, I apprehended that conſiderable difficulties would have occurred, from want of knowing whether the cold of the mixture was ſufficiently great, and when a ſufficient quantity of the quickſilver was frozen; for, in the firſt place, there would be no judging when a ſufficient quantity was frozen without taking out the apparatus now and then to examine it, which could not

be done without a loſs of cold; and what is ſtill worſe, if before the experiment was completed the cold of the mixture was ſo much abated as to become leſs than that of congealing mercury, the frozen quickſilver would begin to melt, and the operator would have no way of detecting it, but by finding that great part of his labour was undone. For this reaſon two other mercurial thermometers were ſent called A and B by Mr. HUTCHINS, the ſcales of which were of wood, for which reaſon I ſhall call them, for ſhortneſs, the wooden thermometers, as I ſhall call the two others the ivory ones, their ſcales being of that material; they were graduated to about 600° below nothing, and their balls were nearly equal in diameter to the ſwelled part of the cylinders, in order that the quickſilver in both ſhould cool equally faſt; and it was recommended to Mr. HUTCHINS to put one of theſe into the freezing mixture along with the apparatus: for then, if the cold of the mixture was ſufficient, both thermometers would ſink faſt till the quickſilver in the cylinder began to freeze, when the ivory thermometer would become ſtationary, but the wooden one would ſtill continue to ſink, on account of the contraction of the quickſilver in its ball by freezing; but if this laſt thermometer, after having continued to ſink for ſome time after the ivory one had become ſtationary, ceaſed at laſt to deſcend, it would ſhew, that the mixture was no longer cold enough to freeze mercury; for as long as that was the caſe, the wooden thermometer would continue to deſcend by the freezing of freſh portions of quickſilver in its ball, but would ceaſe to do ſo as ſoon as the cold was at all leſs than that. As I was afraid, however, that the quickſilver might poſſibly freeze and ſtick tight in the tube of this thermometer, and prevent its ſinking, which would make the cold of the mixture appear too ſmall

when

when in reality it was not, one of these thermometers instead of having a vacuum above the quicksilver as usual, was made with a bulb at top filled with air, in order that the pressure might serve to force down the quicksilver.

If the degree of cold at which mercury freezes had been known, a spirit thermometer would have answered better; but that was the point to be determined.

Another advantage which I expected from the wooden thermometer was, that it would afford a guess when a sufficient quantity of the quicksilver in the cylinder was frozen; for if the cold was continued long enough to make that thermometer sink to near 400° below nothing, I supposed, a very visible portion of the quicksilver would be frozen.

It must be observed, however, that in Mr. HUTCHINS's experiments the natural cold approached so near to the point of mercurial congelation, and in consequence the freezing mixture retained its cold so long as to make these precautions of not so much use as they would otherwise have been.

As it appeared, from Mr. HUTCHINS's table of comparison, that these thermometers did not agree well together, they were all examined after they came back, except the ivory thermometer F, which was broke before it arrived. This loss, however, is of little consequence, as it appeared from the above-mentioned table, that F and G agreed well together. The boiling and freezing points were first examined in the presence of Sir JOSEPH BANKS, Dr. BLAGDEN, Mr. HUTCHINS, Mr. NAIRNE, and myself, when the divisions on the scale answering thereto were found to be as follows:

Boiling

	Boiling point.	Freezing point.
A	220,3	29,9
B	218,8	30,9
G	215,3	32

The boiling point was tried in the manner recommended in the report of the Committee of the Royal Society, printed in the Philoſophical Tranſactions for the year 1777, and allowance made, as there directed, for the height of the barometer at that time. In fixing the freezing point alſo allowance was made for the temperature of the room in which it was tried.

The great difference in the poſition of the boiling point on theſe thermometers ſeems owing only to care not having been taken to keep the quickſilver in the tube of the ſame heat as that in the ball, which is a circumſtance that was very little attended to when they were made; and I am afraid is not ſo much obſerved at preſent as it ought to be, and which in A and B, whoſe tubes contained upwards of 900° of quickſilver, cauſed an exceſſively great error, and much more than it did in G, which contained fewer degrees in its tube.

In order to ſee whether the inequalities of the bore of the tube were properly allowed for, a column of quickſilver, about 100° long, was ſeparated from the reſt; and it was examined, whether its length comprehended the ſame number of degrees on the ſcale in different parts of the tube; when no ſenſible error could be found in this reſpect in G, and none worth regarding in B. The thermometer A, by reaſon of its being conſtructed with a bulb filled with air at top, could not be examined in this manner; but there is no reaſon to think, that it was faulty in this reſpect.

From

From what has been said it appears, that 183°,3 on the scale of G are equal to only 180° on a thermometer adjusted as recommended by the Committee, and therefore 72° are equal to 70°$\frac{2}{7}$; so that the point of −40° answers really to −38°$\frac{2}{7}$; that is, the cold shewn by this thermometer at the temperature of about −40° is 1°$\frac{1}{7}$ too great. In like manner it appears, that the cold shewn at that temperature by B is 4°$\frac{1}{7}$, and by A 6$\frac{1}{7}$, too great.

On the whole, these thermometers seem to have been carefully made, their disagreement being owing only to a faulty manner of adjusting the boiling point, and to not allowing for the temper of the air in settling the degree of freezing; and as these points were examined after they came back, the experiments made with them are just as much to be depended on as if they had been truly adjusted at first.

These instruments were made in the year 1776, and were intended to have been sent to Mr. HUTCHINS that year, through the hands of the late Dr. MATY, who promised to recommend the experiment to him; but, by not being got ready time enough to be sent that year, and a mistaken supposition that Mr. HUTCHINS was to come back the next summer, they were prevented from being sent till 1781; when Sir JOSEPH BANKS was informed by Mr. WEGG, that there was a gentleman at Hudson's Bay who was willing to undertake any experiments of that kind; and that the Hudson's Bay Company would be at the expence of any instruments necessary for the purpose. Then, as Sir JOSEPH thought the abovementioned apparatus well adapted to the purpose, I gladly embraced the opportunity of sending it. It appears, however, from the letter inserted by Mr. HUTCHINS, that Dr. BLACK, without being acquainted

with

with what I had done, recommended nearly the ſame method of determining the degree of cold at which mercury freezes.

Beſides the abovementioned inſtruments, there were ſent to Mr. HUTCHINS two ſpirit thermometers and a thermometer marked C, made at the expence of the Hudſon's Bay Company. The two ſpirit thermometers were made at the recommendation, and under the inſpection of Dr. BLAGDEN, and were of great uſe, as they ſerve to aſcertain ſeveral circumſtances relating to the experiments, which could not otherwiſe have been determined. The intention of the thermometer C will be mentioned in the courſe of this paper.

Before I enter into the examination of Mr. HUTCHINS's experiments, it will be proper to take notice of a phenomenon which occurs in the freezing of water, and is now found to take place in that of quickſilver, and which occaſioned many remarkable appearances in theſe experiments.

It is well known, that if a veſſel of water, with a thermometer in it, is expoſed to the cold, the thermometer will ſink ſeveral degrees below the freezing point, eſpecially if the water is covered up ſo as to be defended from the wind, and care is taken not to agitate it; and then, on dropping in a bit of ice, or on mere agitation, ſpiculæ of ice ſhoot ſuddenly through the water, and the incloſed thermometer riſes quickly to the freezing point where it remains ſtationary *.

This

* Though I here ſay conformably to the common opinion, that mere agitation may ſet the water a freezing, yet ſome experiments, lately made by Dr. BLAGDEN, ſeem to ſhew, that it has not much, if any, effect of that kind, otherwiſe than by bringing the water in contact with ſome ſubſtance colder than itſelf. Though in general alſo the ice ſhoots rapidly, and the incloſed thermometer riſes very quick; yet I once obſerved it to riſe very ſlowly, as, to the beſt of my remembrance, it took up not leſs than half a minute before it roſe to the freezing point;

This ſhews, that water is capable of being cooled conſiderably below the freezing point, without any congelation taking place; and that, as ſoon as by any means a ſmall part of it is made to freeze, the ice ſpreads rapidly through the remainder of the water. The cauſe of the riſe of the thermometer, when the water begins to freeze, is the circumſtance now pretty well known to philoſophers, that all, or almoſt all, bodies by changing from a fluid to a ſolid ſtate, or from the ſtate of an elaſtic to that of an unelaſtic fluid, generate heat; and that cold is produced by the contrary proceſs. This explains all the circumſtances of the phenomenon perfectly well; for as ſoon as any part of the water freezes, heat will be generated thereby in conſequence of the abovementioned law, ſo that the new formed ice and remaining water will be warmed, and muſt continue to receive heat by the freezing of freſh portions of water, till it is heated exactly to the freezing point, unleſs the water could become quite ſolid before a ſufficient quantity of heat was generated to raiſe it to that point, which is not the caſe; and it is evident, that it cannot be heated above the freezing point, for as ſoon as it comes thereto, no more water will freeze, and conſequently no more heat will be generated.

The reaſon why the ice ſpreads all over the water, inſtead of forming a ſolid lump in one part, is, that as ſoon as any ſmall portion of ice is formed, the water in contact with it will be ſo much warmed as to be prevented from freezing; but the water at a little diſtance from it will ſtill be below the freezing point, and will conſequently begin to freeze.

point; but in this experiment the water was cooled not more than one or two degrees below freezing; and it ſhould ſeem, that the more the water is cooled below that point, the more rapidly the ice ſhoots, and the incloſed thermometer riſes.

If it was not for this generation of heat by the act of freezing, whenever a veſſel of water, expoſed to the cold, was arrived at the freezing point, and began to freeze, the whole would inſtantly be turned into ſolid ice; for as the new formed ice is not ſenſibly colder than water beginning to freeze, it follows, that as ſoon as all the water in the veſſel was cooled to that point, the leaſt addition of cold would convert the whole into ice; whereas it is well known, that though the whole veſſel of water is cooled to, or even below, the freezing point, there is a long interval of time between its beginning to freeze and being intirely frozen, during all which time it does not grow at all colder.

In like manner, it is the cold generated by the melting of ice which is the cauſe of the long time required to thaw ice or ſnow. It is this alſo which is the cauſe of the cold produced by freezing mixtures; for no cold is produced by mixing ſnow with any ſubſtance, unleſs part of the ſnow is diſſolved.

I formerly found, by adding ſnow to warm water, and ſtirring it about till all was melted, that the water was as much cooled as it would have been by the addition of the ſame quantity of water, rather more than 150° colder than the ſnow; or, in other words, ſomewhat more than 150° of cold are generated by the thawing of ſnow; and there is great reaſon to think, that juſt as much heat is produced by the freezing of water. The cold generated was exactly the ſame whether I uſed ice or ſnow *.

I have

* I am informed, that Dr. BLACK explains the abovementioned phenomena in the ſame manner; only, inſtead of uſing the expreſſion, heat is generated or produced, he ſays, latent heat is evolved or ſet free; but as this expreſſion relates to an hypotheſis depending on the ſuppoſition, that the heat of bodies is owing to their

I have formerly kept a thermometer in melted tin and lead till they became ſolid; the thermometer remained perfectly ſtationary from the time the metal began to harden round the ſides of the pot till it was intirely ſolid; but I could not perceive it to ſink at all below that point, and riſe up to it when the metal began to harden. It is not unlikely, however, that the great difference of heat between the air and melted metal might prevent this effect from taking place; ſo that though I did not perceive it in thoſe expriments, it is not unlikely that thoſe metals, as well as water and quickſilver, may bear being cooled a little below the freezing or hardening point (for the hardening of melted metals and freezing of water ſeems exactly the ſame proceſs) without beginning to loſe their fluidity.

Mr. HUTCHINS's five firſt experiments were made with the apparatus, and in the manner above deſcribed. In the firſt experiment the ivory thermometer, incloſed in the cylinder, ſunk to $-40°$, where it remained ſtationary for about half an hour, though the wooden thermometer, placed in the ſame mixture, kept ſinking almoſt all the while. At the end of that time the apparatus was taken out of the mixture to be examined, and the quickſilver in the cylinder was found frozen. It ſeems evident, therefore, that the true point at which mercury freezes is 40° below nothing on the thermometer F, which was that made uſe of in the experiment. It cannot be lower than that,

their containing more or leſs of a ſubſtance called the matter of heat; and as I think Sir ISAAC NEWTON's opinion, that heat conſiſts in the internal motion of the particles of bodies, much the moſt probable, I choſe to uſe the expreſſion, heat is generated. Mr. WILKE alſo, in the Tranſactions of the Stockholm Academy of Sciences, explains the phenomena in the ſame way, and makes uſe of an hypotheſis nearly ſimilar to that of Dr. BLACK. Dr. BLACK, as I have been informed, makes the cold produced by the thawing of ſnow 140°; Mr. WILKE, 130°.

for if it was, the thermometer could not have remained ſo long ſtationary at that point, while ſurrounded with freezing quickſilver; and it cannot be higher, as the thermometer could not ſink below the freezing point, while much of the quickſilver, with which it was ſurrounded, remained unfrozen.

To thoſe who have attended to the former part of this paper it is needleſs ſaying, that the reaſon why the wooden thermometer continued ſinking ſo long after the ivory thermometer became ſtationary is, that as the former was placed in the freezing mixture, the quickſilver in its ball froze, and therefore it continued deſcending during the greateſt part of that half hour, by the continual freezing of freſh portions of quickſilver in its ball, and the contraction occaſioned thereby; whereas the latter, which was placed only in freezing quickſilver, did not freeze.

There is a circumſtance, however, in this experiment, the reaſon of which does not ſo readily appear; namely, on putting back the apparatus into the freezing mixture, after it was taken out to be examined, the thermometer ſunk to $-42°$; but in about four or five minutes returned back to $-40°$. The like happened on removing the apparatus into a freſh freezing mixture, and it then remained about ten minutes before it returned to $-40°$. It ſeems probable from this, that the quickſilver in the cylinder became intirely frozen about the time that it was firſt taken out to be examined, and that it then grew 2° colder than the freezing point; and that this degree of cold was not ſufficient to make the quickſilver in the incloſed thermometer freeze, ſince mercury, as was before ſaid, will bear being cooled a little below its freezing point without freezing. What confirms this explanation is, that the ſpirit thermometers ſhew that the cold of the mixture was actually much the ſame as that ſhewn by the ivory thermometer.

In

In the ſecond experiment, tried with the ſame apparatus, the ivory thermometer quickly ſunk to $-43°$; but, in about half a minute, roſe to $-40°$, where it remained ſtationary for upwards of 17′. It appears, therefore, that in this experiment the quickſilver was cooled 3° below the freezing point, without loſing its fluidity; it then began to freeze, and the incloſed thermometer immediately roſe to $-40°$: ſo that this experiment, beſides confirming the former, ſhews, that quickſilver is capable of being cooled a little below the freezing point without freezing; and that it ſuddenly riſes up to it as ſoon as it begins to loſe its fluidity.

In this experiment the cold was carried far enough to freeze the quickſilver in the ivory thermometer, which was not the caſe in the former: for after it had remained 17′ ſtationary at $-40°$, it began to ſink again, and in about a minute ſunk to $-44°\frac{1}{2}$; it then ſunk inſtantaneouſly to $-92°$, and ſoon after remained fixed for an hour and a quarter at 95°; being then left without examination for three-quarters of an hour, the mercury was found to have ſunk into the ball, the ſpirit thermometer ſhewing at that time that the mixture was rather above the point of freezing, whereas before it had been below it. It appears, therefore, that the quickſilver in the thermometer, after having deſcended to $-44°\frac{1}{2}$, froze in the tube, and ſtuck there; but, being by ſome means looſened, ſunk inſtantly to $-92°$, and again ſtuck tight at $-95°$, till at laſt the mixture riſing above the freezing point, the quickſilver in the tube melted, and ſunk into the ball, to ſupply the vacuum formed there by the frozen quickſilver. A ſimilar accident of the quickſilver freezing in the tube of the thermometer, and ſticking there, and then melting and ſinking into the ball as the weather grew warmer, has been found by Dr. BLAGDEN to have

have happened to ſeveral gentlemen whoſe thermometers froze by the natural cold of the atmoſphere, and with reaſon cauſed much perplexity to ſome of them.

In this experiment the apparatus was not taken out to be examined till the ivory thermometer had ſunk to $-95°$; it was then found to be frozen ſolid.

The third experiment was tried while the former was carrying on, and was made by putting the other apparatus, namely, that with the thermometers G and B, into the firſt mixture made for the former experiment, and which may conſequently be ſuppoſed to have loſt great part of its cold. The ivory thermometer quickly ſunk to $-43°$, where it remained ſtationary for near 12′. The apparatus being then taken out to be examined, the quickſilver in the cylinder was found fluid, but thick and in grains, like crumbs of bread. The apparatus was then put back into the mixture; and, on obſerving the thermometer, it was found to have riſen to $-40°$, where it remained ſtationary about 40′; being then examined, the quickſilver was found ſolid.

It appears, therefore, that the cold of the mixture was ſufficient to cool the quickſilver in the cylinder about 3° below the point of freezing, but did not make it freeze till, on taking out the apparatus, the agitation ſuddenly ſet it a freezing, and produced the appearance deſcribed by Mr. HUTCHINS. This immediately made the incloſed thermometer riſe; ſo that when it was re-placed in the mixture and obſerved, it ſtood exactly at the freezing point. It appeared, by the ſpirit thermometer, that the cold of the mixture, at the time the apparatus was firſt taken out to be examined, was only 2° below the point of freezing, which agrees very well with this explanation.

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This experiment, therefore, affords a freſh confirmation that the point of mercurial congelation is $-40°$ on theſe thermometers; and that quickſilver will bear being cooled a little below that point without freezing.

As in theſe two experiments the quickſilver in the cylinder and ivory thermometer bore being cooled a few degrees below the freezing point without freezing, it is natural to conclude, that the ſame fluid in the wooden thermometer ſhould do ſo too; and it may, perhaps, be ſuppoſed that, in conſequence of it, this thermometer, after having ſunk a little below the point of freezing, ought ſuddenly to have riſen up to it, which was not obſerved. But there is great reaſon to think, that though the quickſilver in it did bear cooling in this manner, it would not have occaſioned any ſuch appearance: for ſuppoſe that it is cooled below the freezing point, and then ſuddenly freezes, its bulk will be increaſed, on account of the heat generated thereby; but then it will be diminiſhed on account of the contraction in freezing; ſo that, unleſs the expanſion by the heat generated exceeds the contraction by freezing it will cauſe no riſe in the thermometer. I do not, indeed, know how much the heat generated by freezing in quickſilver is, but in water it is about 150°, and the contraction by freezing is at leaſt as much as its expanſion by 400°; ſo that, unleſs the heat generated by freezing is two or three times as great in quickſilver as in water, the thermometer ought not to riſe on this account.

In the fourth, fifth, ſixth, and ſeventh experiments a new phenomenon occurred, namely, the ivory thermometer ſunk a great deal below the freezing point without ever becoming ſtationary at $-40°$. In the fifth experiment, tried with the apparatus G, it quickly ſunk to $-42°$, and then, without remaining ſtationary at any point, ſunk in half a minute to

$-72°$, and ſoon after remained fixed at $-79°$. While it was at $-79°$, the apparatus was twice examined, and the quickſilver found fluid; but being again examined after having been removed into a freſh mixture, it was found ſolid.

It ſeems likely from hence, that the quickſilver, in the cylinder was quickly cooled ſo much below the freezing point as to make that in the incloſed thermometer freeze, though it did not freeze itſelf. If ſo, it accounts for the appearances perfectly well; nor does there ſeem any thing improbable in the explanation, except that it is contrary to what happened in the three firſt experiments; but the degree to which fluids will bear being cooled below the freezing point without freezing ſeems to depend on ſuch minute circumſtances, that, I think, this forms no objection. It muſt be obſerved, that the cold of the mixture appeared by the ſpirit thermometer to be five or ſix degrees below the freezing point; ſo that if the quickſilver in the cylinder was as cold as the mixture, and I have no reaſon to think it was not, it is not at all extraordinary that the thermometer ſhould have froze; the only thing extraordinary is, that the quickſilver in the cylinder ſhould have borne that cold without freezing.

The ſame phenomenon occurred in the ſixth and ſeventh experiments, on putting the ſame apparatus into the freezing mixture.

In the fourth experiment the ivory thermometer ſunk quickly to $-42°$; but ſoon after roſe half a degree, probably from the cold of the mixture diminiſhing; it then, after having remained ſix or ſeven minutes at thoſe two points, ſunk very quick to $-77°$. It does not appear, at what time the quickſilver in the cylinder began to freeze, as it was not examined till long after the thermometer had ſunk to $-77°$, when it was

found ſolid; but from the reſemblance of this to the three former experiments, I think it much moſt likely, that it did not begin to freeze till after the thermometer had ſunk to $-77°$.

In the fifth experiment the wooden thermometer was partly frozen before it was put into the freezing mixture, and the ivory one was at $-40°$. On putting them into the mixture, they both roſe; the latter, half a degree; the former, many degrees; which ſhews that the part of the mixture in which they were placed was rather warmer than the freezing point, though that in which the ſpirit thermometer was placed was colder; but as there ſeems nothing to be learnt from this, it is not worth while entering into a detail of the circumſtances.

Though theſe experiments do not ſerve to ſhew what the freezing point of quickſilver is, yet they do not at all contradict the concluſion drawn from the three former.

If theſe experiments only had been made, I ſhould have been inclined to ſuppoſe, that quickſilver froze with a leſs degree of cold in vacuo than in the open air, as the quickſilver in the ivory thermometer was in vacuo, and that in the cylinder was not; but, as in the three former experiments, the event was different, the quickſilver in the cylinder there freezing firſt, I have no reaſon to think that this is the caſe.

Though in the ſixth experiment the thermometer in the apparatus G froze without the quickſilver with which it was ſurrounded freezing, yet in trying the apparatus F in the ſame mixture, this did not happen; but, on the contrary, it afforded as ſtriking a proof that the point of freezing quickſilver anſwers to about $-40°$ on this thermometer as any of Mr. HUTCHINS's experiments; for, on taking out the apparatus after it had been two minutes in the mixture, the quickſilver in the cylinder was found frozen ſolid, the incloſed ther-

mometer ſtanding at 40° or 41° below nothing. After having been expoſed for near an hour to the air, which was then very little above the point of freezing quickſilver, only a ſmall quantity of the ſurface was become fluid; the reſt formed a frozen globe round the ball of the thermometer, reſembling poliſhed ſilver, and in 17′ after this only a ſegment of a globe of frozen quickſilver, with a concavity on the inſide, formed by the ball of the thermometer, was obſerved, the thermometer all this while continuing the ſame as before, namely, at 40° or 41° below nothing; ſo that in this experiment the ball of the thermometer was ſurrounded for more than an hour with quickſilver, which was viſibly frozen and ſlowly melting, and during all which time it continued ſtationary at 40° or 41° below nothing.

It muſt be obſerved, however, that in the firſt and ſecond experiments, which were both tried with this apparatus, the freezing point came out exactly −40°, whereas in this it ſeemed about half a degree lower; the reaſon of which, in all probability, is, that the tube of this thermometer was not ſo well fitted to its ſcale but that it had a little play, which would make the freezing point appear near half a degree higher or lower, according as the tube was puſhed up or down.

Though the foregoing experiments leave no reaſonable room to doubt, that this is the true point at which quickſilver freezes, yet Mr. HUTCHINS has, if poſſible, made this ſtill more evident by his two laſt experiments; as, in the firſt of them, he froze ſome quickſilver in a gally-pot immerſed in a freezing mixture, ſo that the quickſilver was in contact with, and covered by, the ſnow and ſpirit of nitre; and in the latter in the open air, by the natural cold of the weather, and then dipping the ball of the thermometer into the unfrozen part, obſerved

what degree it ſtood at. Theſe experiments agree with the former in ſhewing the freezing point to be $-40°$ on the two mercurial thermometers; and alſo ſhew what degree on the ſpirit thermometers anſwers thereto, namely, $29°\frac{1}{4}$ or $28°\frac{1}{4}$ on D, and $30°$ on E; for in theſe two experiments the ſpirit thermometers alſo were dipped into the frozen quickſilver.

In all the experiments, therefore, tried with the thermometer G, the freezing point came out $-40°$. In thoſe tried with F, it came out either $-40°$, or about $-40°\frac{1}{4}$; ſo that as it appears, from Mr. HUTCHINS's table of compariſon, that F ſtood at a medium a quarter of a degree lower than G, the experiments made with that thermometer alſo ſhew the freezing point to be $-40°$ on G; and as it appeared from the examination of this thermometer after it came home, that $-40°$ thereon anſwers to $-38\frac{2}{11}$, on a thermometer adjuſted in the manner recommended by the Committee of the Royal Society, it follows, that all the experiments agree in ſhewing that the true point at which quickſilver freezes is $38°\frac{2}{11}$, or in whole numbers $39°$ below nothing.

From what has been ſaid it appears, that the point at which quickſilver freezes has been determined by Mr. HUTCHINS in different ways, all perfectly ſatisfactory, and all agreeing in the ſame reſult. In the three firſt experiments the thermometer was ſurrounded by quickſilver, which continued freezing till it became ſolid. In the ſixth experiment the quickſilver with which it was ſurrounded continued ſlowly melting till the whole was diſſolved; and in both caſes the thermometer remained ſtationary all the while at what we have juſt ſaid to be the freezing point. In the ninth and tenth experiments, the ball of the thermometer was dipped into quickſilver, previouſly frozen and beginning to melt, as uſually practiſed in ſettling the

freezing point on thermometers, and agreed in the same result, the quicksilver in the last experiment being frozen by the natural cold of the atmosphere; and in the former, by being immersed in, and in contact with, a freezing mixture; so that this point appears to be determined in as satisfactory a manner as can be desired; and the more so, as it seems impossible that experiments should be made with more care and attention, or more faithfully and circumstantially related than these have been. The second and third experiments also shew, that quicksilver, as well as water, can bear being cooled a little below the freezing point without freezing, and is suddenly heated to that point as soon as it begins to congeal.

On the contraction of quicksilver in freezing.

All these experiments prove, that quicksilver contracts or diminishes in bulk by freezing; and that the very low degrees to which the thermometers have been made to sink, is owing to this contraction, and not to the cold having been in any degree equal to that shewn by the thermometer. In the fourth experiment the thermometer A sunk to $-450°$, though it appeared by the spirit thermometers that the cold of the mixture was not more than 5° or 6° below the point of freezing quicksilver. In the first experiment also, it sunk to $-448°$, at a time when the cold of the mixture was only $2°\frac{1}{4}$ below that point; so that it appears, that the contraction of quicksilver, by freezing, must be at least equal to its expansion by 404° of heat*. This, how-

* The numbers here given are those shewn by the thermometer without any correction; but if a proper allowance is made for the error of that instrument it will appear, that the true contraction was 25° less than here set down, and from the manner in which thermometers have been usually adjusted, it is likely, that in the following experiment of Mr. HUTCHINS, as well as those of Professor BRAUN, the true contraction might equally fall short of that shewn by observation.

ever, is not the whole contraction which it ſuffers; for it appears, by an extract which Mr. HUTCHINS was ſo good as to give me from a meteorological journal, kept by him at Albany Fort, that his thermometer once ſunk to 490° below nothing, though it appeared, by a ſpirit thermometer, that the cold ſcarcely exceeded the point of freezing quickſilver. There are two experiments alſo of Profeſſor BRAUN, in which the thermometer ſunk to 544° and 556° below nothing, which is the greateſt deſcent he ever obſerved without the ball being cracked. It is not indeed known how cold his mixtures were; but from Mr. HUTCHINS's, there is great reaſon to think that they could not be many degrees below −40°. If ſo, the contraction which quickſilver ſuffers in freezing is ſometimes not much leſs than its expanſion by 500° or 510° of heat, that is almoſt $\frac{1}{50}$d of its whole bulk, and in all probability is never much more than that.

It is very likely, however, that the contraction which quickſilver ſuffers in freezing is no very determinate quantity; for a conſiderable difference may frequently be obſerved in the ſpecific gravity of the ſame piece of metal, caſt different times over, and almoſt all caſt metals become heavier by hammering; and it is likely that the ſame thing may obtain in quickſilver, which is only a metal which melts with a much leſs degree of heat than the reſt. I do not know, indeed, how much this variation can amount to; but, on caſting the ſame piece of tin three times over, I found its denſity to vary from 7,252 to 7,294, though I have great reaſon to think that no hollows were left in it, and that only a ſmall part of this difference could proceed from the error of the experiment. This variation of denſity is as much as is produced in quickſilver by an alteration of 66° of heat; and it is not unlikely, that the deſcent of a thermometer, on account of the contraction of the quickſilver in its ball by freezing,

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may vary as much in different trials, though the whole maſs of quickſilver is frozen and without any vacuities.

The thermometer marked C was intended for trying how much the contraction of quickſilver is; but the experiments made with it were not attended with ſucceſs, as in the firſt experiment it did not ſink ſo low as A had done, owing, moſt likely, to the great cold of the weather which froze the quickſilver in the tube; and in the ſecond experiment the ball broke.

On the cold of the freezing mixtures.

The cold produced by mixing ſpirit of nitre with ſnow is owing, as was before ſaid, to the melting of the ſnow. Now, in all probability, there is a certain degree of cold in which the ſpirit of nitre, ſo far from diſſolving ſnow, will yield out part of its own water, and ſuffer that to freeze, as is the caſe with ſolutions of common ſalt; ſo that if the cold of the materials before mixing is equal to this, no additional cold can be produced. If the cold of the materials is leſs, ſome increaſe of cold will be produced; but the total cold will be leſs than in the former caſe, ſince the additional cold cannot be generated without ſome of the ſnow being diſſolved, and thereby weakening the acid, and making it leſs able to diſſolve more ſnow; but yet the leſs the cold of the materials is, the greater will be the additional cold produced. This is conformable to Mr. HUTCHINS's experiments; for in the fifth experiment, in which the cold of the materials was $-40°$, the additional cold produced was only $5°$. In the firſt experiment, in which the cold of the materials was only $-25°$, an addition of at leaſt $19°$ of cold was obtained; and by mixing ſome of the ſame ſpirit of nitre with ſnow in this climate, when the heat of the materials

materials was + 26°, I have ſunk the thermometer to − 29°; ſo that an addition of 55° of cold was produced.

It is remarkable, that in none of Mr. HUTCHINS's experiments the cold of the mixture was more than 6° of the ſpirit thermometer below the point of freezing quickſilver, which is ſo little that it might incline one to think, that the ſpirit of nitre uſed by him was weak. This, however, was not the caſe, as its ſpecific gravity at 58° of heat was 1,4923. It was able to diſſolve $\frac{1}{1,42}$ its weight of marble, and contained very little mixture of the vitriolic or marine acid: as well as I could judge from what experience I have of ſpirit of nitre, it was as little phlogiſticated as acid of that ſtrength uſually is.

But, however extraordinary it may at firſt appear, there is the utmoſt reaſon to think, that a rather greater degree of cold would have been obtained if the ſpirit of nitre had been weaker; for I found, by adding ſnow gradually to ſome of this acid, that the addition of a ſmall quantity produced heat inſtead of cold; and it was not until ſo much was added as to increaſe the heat from 28° to 51°, that the addition of more ſnow began to produce cold; the quantity of ſnow required for this purpoſe being pretty exactly one-quarter of the weight of the ſpirit of nitre, and the heat of the ſnow and air of the room, as well as of the acid, being 28°. The reaſon of this is, that a great deal of heat is produced by mixing water with ſpirit of nitre, and the ſtronger the ſpirit is, the greater is the heat produced. Now it appears from this experiment, that before the acid was diluted, the heat produced by its union with the water formed from the melted ſnow was greater than the cold produced by the melting of the ſnow; and it was not till it was diluted by the addition of one-quarter of its weight of

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that ſubſtance, that the cold generated by the latter cauſe began to exceed the heat generated by the former. From what has been ſaid it is evident, that the cold of a freezing mixture, made with the undiluted acid, cannot be quite ſo great as that of one made with the ſame acid, diluted with a quarter of its weight of water, ſuppoſing the acid and ſnow to be both at 28° of heat, and there is no reaſon to think, that the event will be different if they are colder; for the undiluted acid will not begin to generate cold until ſo much ſnow is diſſolved as to increaſe its heat from 28° to 51°, ſo that no greater cold will be produced than would be obtained by mixing the diluted acid heated to 51° with ſnow of the heat of 28°. This method of adding ſnow gradually to an acid is much the beſt way I know of finding what ſtrength it ought to be of, in order to produce the greateſt effect poſſible.

By means of this acid, diluted in the above-mentioned proportion, I froze the quickſilver in the thermometer called G by Mr. HUTCHINS, on the 26th of laſt February. I did not, indeed, break the thermometer to examine the ſtate of the quickſilver therein; for as it ſunk to − 110° it muſt certainly have been in part frozen; but immediately took it out, and put the ſpirit thermometer in its room, in order to find the cold of the mixture. It ſunk only to − 30°; but, by making allowance for the ſpirit in the tube being not ſo cold as that in the ball, it appears, that if it had not been for this cauſe it would have ſunk to − 35°*, which is 5° below the point of freezing, and is

* As the ſurface of the freezing mixture anſwered to − 185° on the tube, there were 155° of ſpirit in the tube which could hardly be cooled much below the temper of the air, and which muſt, therefore, be warmer than that in the ball by about 55° of this thermometer, as the heat of the ſpirit in the ball was before ſaid

is as great a degree of cold, within 1°, as was produced in any of Mr. HUTCHINS's experiments.

In this experiment the thermometer G ſunk very rapidly, and, as far as I could perceive, without ſtopping at any intermediate point, till it came to the above-mentioned degree of −110°, where it ſtuck. The materials uſed in making the mixture were previouſly cooled, by means of ſalt and ſnow, to near nothing; the temper of the air was between 20° and 25°; the quantity of acid uſed was 4¼ oz.; and the glaſs in which the mixture was made was ſurrounded with wool, and placed in a wooden box, to prevent its loſing its cold ſo faſt as it would otherwiſe have done.

Some weeks before this, I made a freezing mixture with ſome ſpirit of nitre, much ſtronger than that uſed in the foregoing experiment, though not quite ſo ſtrong as the undiluted acid, in which the cold was leſs intenſe by 4°½, as the thermometer G ſunk to −40°½. It is true, that the temper of the air was much leſs cold, namely, 35°; but the ſpirit of nitre was at leaſt as cold, and the ſnow not much leſs ſo. The experiment was tried in the ſame veſſel and with the ſame precautions as the former.

The cold produced by mixing oil of vitriol, properly diluted with ſnow, is not ſo great as that procured by ſpirit of nitre, though it ſeems not to differ from it by ſo much as 8°; for a freezing mixture, prepared with diluted oil of vitriol, whoſe

ſaid to be −35°, and the temper of the air above +20. Therefore, the correction muſt be equal to the expanſion of a column of ſpirits 155° long, by an alteration of heat equal to 55° on this thermometer, which, if 1° on the ſcale anſwers to $\frac{1}{1700}$th of the bulk of the ſpirit, is equal to $\frac{55 \times 155}{1700}$ or 5°.

ſpecific gravity, at 60° of heat, was 1,5642, ſunk the thermometer G to −37°, the experiment being tried at the ſame time, and with the ſame precautions, as the foregoing. It was previouſly found, by adding ſnow gradually to ſome of this acid, as was done by the ſpirit of nitre, that it was a little, but not much ſtronger than it ought to be, in order to produce the greateſt effect.

XXI. *History of the Congelation of Quickſilver.* By Charles Blagden, *M. D. F. R. S. Phyſician to the Army.*

Read June 5, 1783.

THE late experiments at Hudſon's Bay have determined a point, upon which philoſophers not only were much divided in their opinion, but alſo entertained, in general, very erroneous ſentiments. Though many obvious circumſtances rendered it improbable, that the term of mercurial congelation ſhould be five or ſix hundred degrees below 0 of FAHRENHEIT's ſcale, as had been at firſt ſuppoſed; yet ſcarcely any one ventured to imagine that it was ſhort of 100°. Mr. HUTCHINS, however, has clearly proved, that even this number is far beyond the truth; and that quickſilver freezes in a degree of cold not exceeding that which ſometimes occurs in the northern parts of Europe, and frequently in the more rigorous climates of Aſia and America. It now appears, that quickſilver, ſo far from containing any eſſential principle of fluidity, does not differ from ſome of the other metals, in its melting point, nearly ſo much as they differ among themſelves; and as it is malleable in its ſolid ſtate, and after calcination recovers its metallic form by heat alone, without the addition of inflammable matter, there can be no doubt but it muſt be ranked among the perfect metals, which, therefore, arranged accord-

ing to their ſpecific gravity, are theſe four: platina*, gold, quickſilver, and ſilver.

In the general progreſs of ſcience, it is always uſeful at intervals, and eſpecially when any conſiderable advance has been made, to look round and contemplate the proſpect left behind. Thus our actual ſituation is more diſtinctly comprehended, and a better judgement may be formed of what remains to be done. For this reaſon, I thought it might not be unacceptable to the Society, if I were, on the preſent occaſion, to lay before them an account of the different obſervations and experiments I have been able to collect, relative to the congelation of quickſilver; eſpecially as many of theſe are recorded in books not eaſily procured, and in languages little underſtood by the learned of this country. I ſhall begin with the various attempts which have been made to render this metal ſolid by frigorific mixtures, and then enumerate the many inſtances in which that effect has certainly been produced by the natural cold of the air.

PART I.

§ 1. IT was undoubtedly M. JOSEPH ADAM BRAUN, Profeſſor of Philoſophy in the Imperial Academy at Peterſburg, who firſt, upon deciſive evidence, eſtabliſhed the fact, that quickſilver can be made ſolid by a diminution of its heat: but,

* Unleſs, indeed, the irreducible black powder, obtained by M. TILLET, ſhould be thought to place platina among the imperfect metals. See Mem. de l'Ac. Roy. des Scienc. 1779, p. 404, &c.

as in moſt diſcoveries, much depends upon accident, ſo, on this occaſion, M. BRAUN undertook the experiments for a very different object from that which preſented itſelf in the courſe of them, and at the ſuggeſtion of another perſon. This gentleman was Dr. JOHN ERNEST ZEIHER, Profeſſor of Mechanics in the ſame Academy, who having repeated FAHRENHEIT's experiments with frigorific mixtures in Germany, before he came to ſettle at Peterſburg, wiſhed to try whether they might not be proſecuted further in the great natural cold which ſometimes prevails in that city. Illneſs prevented Dr. ZEIHER from carrying his ideas into execution; he therefore communicated them to Profeſſor BRAUN, who was already much converſant in thermometrical experiments, and engaged him to take up the ſubject of artificial cold whenever the weather ſhould be favourable for this purpoſe. A proper opportunity occurred on the 14th of December, 1759, O. S. the thermometer ſinking in the open air ſo low as $-34°$ of FAHRENHEIT's ſcale, which we now know to be within a few degrees of the point at which mercury freezes. M. BRAUN accordingly prepared a frigorific mixture with *aqua fortis* and pounded ice, by means of which his thermometer was reduced to $-69°$, lower, by almoſt 30 degrees, than it had fallen in any preceding experiments of this nature.

Part of the quickſilver had now really congealed, yet ſo far was M. BRAUN from entertaining a ſuſpicion of that ſort, and ſo entirely were his views directed to another object, that he acknowledges he had well nigh deſiſted from all further trials, content with having thus much exceeded ſuch eminent philoſophers as FAHRENHEIT, MUSSCHENBROECK, and REAUMUR.

Animated, however, by the hope that a ſtill greater degree of cold might be produced, he entered upon the experiment anew;

anew; and all his pounded ice being expended, he was fortunately obliged to ſubſtitute ſnow in its place. With this freſh mixture he had the ſatisfaction of ſeeing the mercury in his thermometer ſink to $-100°$, and in ſucceſſive experiments to $-244°$ and $-352°$. Surpriſed at ſo unexpected an event, he drew the inſtrument out of the mixture, and carefully examined its bulb, to ſee if it had received any injury; but he found it perfectly entire, and moreover perceived a much more unexpected phænomenon, that the quickſilver was fixed, and remained immoveable above 12 minutes. On repeating the ſame experiment with another thermometer, graduated no lower than $-220°$, all the mercury ſunk into the ball, and became ſolid as before, not beginning to re-aſcend till after a ſtill longer interval of time.

From theſe appearances the profeſſor very juſtly concluded, that the quickſilver in both inſtruments had been fixed or frozen by the cold; but as the evidence was not yet complete, he only ventured to propoſe the congelation of mercury as a *probable truth*, at the next meeting of the Academy held three days afterwards; and in the mean time was making preparations to acquire more palpable proofs of the fact. The thermometers ordered with this view were not ready till the 25th of December O. S. when, in company with the celebrated ÆPINUS, Profeſſor of Phyſics, he performed the experiment with ſimilar materials, and as ſoon as he found the quickſilver immoveable, broke the bulb of his thermometer. Now all his doubts were removed; he obtained a ſolid ſhining metallic maſs, which extended under the ſtrokes of a peſtle, in hardneſs rather inferior to lead, and yielding a dull dead ſound like that metal. Profeſſor ÆPINUS was occupied at the ſame time in ſimilar experiments, employing both thermometers and ſimple

tubes

tubes of a large bore; with which laſt he remarked, that the quickſilver in them fell ſenſibly on freezing, and aſſumed a concave ſurface; likewiſe, that the congealed pieces would ſink in fluid mercury; all evident proofs of its great contraction. Theſe obſervations were frequently repeated during the winter, with ſome variety in the circumſtances and phænomena, by Profeſſor BRAUN and many other perſons; I find M. LOMONOSOV, Profeſſor of Chemiſtry, the Apothecary-general MODEL, Meſſ. KRASE, HIMSEL, and POISSONNIER, quoted on different occaſions as witneſſes, they having all either performed or aſſiſted at the experiments. Such evidence one would have imagined ſufficient to place the fact beyond all controverſy, and render the congelation of mercury one of the moſt acknowledged truths in natural philoſophy.

It may not here be improper to remark, as an additional proof how much we are indebted to accident for diſcoveries, that if Mr. BRAUN had chanced to begin with a ſpirit thermometer inſtead of a mercurial one, we might very poſſibly to this day have remained ignorant that quickſilver would freeze. For ſince, to judge from Mr. HUTCHINS's experiments, the former would have ſunk but a few degrees in the frigorific mixture, it is not improbable, that the profeſſor, diſcouraged by ſuch a failure of ſucceſs in the immediate object of his purſuit, would have relinquiſhed all further attempts of this nature.

When the ſeaſon for experiments requiring cold was paſt, Profeſſor BRAUN employed himſelf in drawing up a general account of ſuch as he had then made, which he communicated to the Peterſburg Academy on the 6th of September, 1760, O. S. and printed ſoon afterwards as a ſeparate diſſertation*. Of this ſo copious an extract, by Dr. WATSON, is already in-

* De admirando frigore artificiali diſſertatio.

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ſerted in the Philoſophical Tranſactions*, that it would be improper for me to dwell upon any further particulars. I will only obſerve, that though many of the circumſtances mentioned by M. BRAUN were not firſt remarked by himſelf, yet the diſſertation is compoſed entirely in his name, all the other gentlemen very generouſly giving up their part to him who made the original diſcovery, and undertook to put the whole in a fit dreſs to appear before the world.

Five years afterwards, Profeſſor BRAUN again addreſſed the publick on the ſame ſubject, under the title of "Supplements" to his former diſſertation †. Here he declares, that ſince the firſt diſcovery he has ſuffered no winter to elapſe without making ſimilar experiments, and never failed of ſucceſs in freezing the quickſilver, whenever there was a proper degree of natural cold, which he ſtates at $-10°$, in order for the experiment to be complete, though ſome commencement of congelation might be perceived when the temperature of the air is as high as $+2°$. He confirms all his former obſervations, and adds many others to illuſtrate them; among which two are very important, as coming nearer than any yet known to aſcertain the real contraction that quickſilver ſuffers in becoming ſolid. At the ſame time it muſt be confeſſed, he has not rectified any of his former miſtakes: he retains the ſame groundleſs opinions relative to the freezing point of the quickſilver, the prodigious cold generated by his mixtures, and the explanation of various phenomena, which depend upon very different principles, from thoſe to which he aſſigns them.

* Vol. LII. p. 156.

† Supplementa de Congelatione Mercurii. Nov. Comment. Acad. Scient. Imperial. Petropol. tom. XI. p. 302. & ſeqq.

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The general ſtate of M. BRAUN's experiments is, that with the above-mentioned frigorific mixtures, and once, when the natural cold was at $-28°$, with rectified ſpirits and ſnow, he congealed the quickſilver, and diſcovered moſt of its properties in a ſolid ſtate, eſpecially that it is a real metal, which melts with a very ſmall degree of heat. But not perceiving the neceſſary conſequence of its great contraction in freezing, though aware of the fact, he perpetually confounded the diminution of its volume from this cauſe with that which is ſimply the effect of cold. Hence he conſidered, as the commencement of congelation, what was, in reality, its extreme term, or the utmoſt contraction which the whole would ſuffer in becoming ſolid. To this, indeed, he ſcarcely ever attained, owing to the various impediments that occurred from adheſion of the quickſilver in the thermometrical tube, hollows left in the bulb as it froze, portions of the mercury remaining uncongealed, and many other cauſes. All theſe being by their nature very irregular, his ſuppoſed freezing point came to be extremely uncertain, and ſeveral anomalous appearances were occaſioned, which could not poſſibly be explained upon his miſtaken ſuppoſition; but, notwithſtanding ſuch errors, the greateſt part of our preſent knowledge on the ſubject of mercurial congelation is to be found in the writings of M. BRAUN, who may therefore juſtly be ſtiled the father of this branch of ſcience.

In his ſupplementary treatiſe, the Profeſſor engages to continue his reſearches, and to lay the reſult of them before the Academy, if they ſhould lead to any thing new. But he did not live to accompliſh his deſign. His original diſſertation was re-printed, and the ſupplement firſt publiſhed, in the XIth tome of the Novi Commentarii Academiæ Scientiarum Petropolitanæ for the year 1765. This volume did not appear till 1767,

and Profeſſor BRAUN died the following year. Theſe two treatiſes contain the ſubſtance of all his obſervations; but ſeveral further particulars, relative to the diſcovery, may be collected from the Philoſophical Tranſactions *, the Hiſtory of the French Academy of Sciences †, and other literary publications of that period.

§ 2. Profeſſor BRAUN, in his firſt diſſertation, expreſſed a very commendable wiſh, that the experiment of congealing quickſilver might be repeated in other countries, and laments, in the ſupplement, that nothing of this kind had been done. It was not, however, till the year 1774, that his aſſertions received any ſort of confirmation out of Ruſſia, and then by a mode of experiment which did not ſeem to promiſe much ſucceſs. M. JOHN FREDERIC BLUMENBACH, then a ſtudent of Phyſic at Gottingen, now Profeſſor of Medicine in the ſame Univerſity, obſerving the intenſe cold that prevailed there in the month of January that year, took the opportunity of expoſing ſome quickſilver to its action. As the original account of this experiment was given only in a ſingle number of the Literary Journal of Gottingen ‡, in the German language, I will here tranſlate it as exactly as poſſible, ſeveral of the circumſtances being very remarkable.

"On the 11th of January," ſays M. BLUMENBACH, "at "half after five in the evening, I put three drams of quick-"ſilver in a ſmall ſugar-glaſs, and covered it with a mixture of "equal parts of ſnow and Egyptian ſal ammoniac. This mix-"ture was put looſe into the glaſs, ſo that the quickſilver lay

* Vol. LI. p 679.

† Hiſt. de l'Académie des Sciences, 1760, p. 26.

‡ Göttingiſche Anzeigen von gelehrten Sachen. Stück 13. Jan. 29, 1774.

"perfectly

" perfectly free, being only covered by it as with pieces of ice;
" the whole, together with the glaſs, weighed ſomewhat above
" an ounce. I hung it out at a window three ſtories high,
" upon a ſmall roof facing the weſt, ſo that the glaſs was
" freely expoſed to the north-weſt; and I mixed with the ſnow
" upon which it ſtood two drams more of ſal ammoniac. The
" ſnow and ſal ammoniac in the glaſs ſoon froze in the open
" air to a maſs like ice: no ſenſible change, however, appeared
" in the quickſilver that evening; but at one in the morning it
" was found frozen ſolid. It had divided into two large and
" four ſmaller pieces; of the former, one was hemiſpherical
" and the other cylindrical, each ſeemingly rather above a dram
" in weight; the four ſmall bits might amount to half a ſcruple.
" They were all with their flat ſide frozen hard to the glaſs, and
" no where immediately touched by the mixture; their colour
" was a dull pale white, with a bluiſh caſt, like zinc, very
" different from the natural appearance of quickſilver. I wiſhed
" much to break the glaſs immediately, and to try how theſe
" bodies would bear the hammer; but deſiring rather to have
" witneſſes of ſuch a rare phænomenon I refrained. The ſpirit
" of wine, in an excellent thermometer made by BRANDER,
" ſtood at this time 10° under 0 of FAHRENHEIT's ſcale, which
" was the cold of Upſal in 1740. Next morning, the 12th,
" about ſeven o'clock, I found that the larger hemiſphere be-
" gan to melt, perhaps becauſe it was moſt expoſed to the air,
" and not ſo near as the others to the ſal ammoniac mixture
" which lay beneath. In this ſtate it reſembled an amalgam,
" ſinking to that ſide on which the glaſs was inclined, but
" without quitting the ſurface of the glaſs, to which it was
" ſtill firmly congealed; the five other pieces had not yet un-
" dergone any alteration, but remained frozen hard, as I had

"obſerved them in the night. I now made haſte to call ſome "of my friends, who came in time to ſee evidently all this "with me. They were the younger Dr. VOGEL, and Meſſ. "WEBER, WAGNER, and GRAUMANN. Toward eight o'clock "the cylindrical piece began to ſoften in the ſame manner as "the former, and the other four ſoon followed. About eight "they fell from the ſurface of the glaſs, and divided into many "fluid ſhining globules, which were ſoon loſt in the inter-"ſtices of the frozen mixture, and re-united in part at the "bottom, being now exactly like common quickſilver."

In this experiment at Gottingen it is not a little ſurpriſing, that the cold ſhould have proved ſufficient to effect ſuch a congelation of the quickſilver. A mercurial thermometer would probably have ſtood lower than M. BLUMENBACH's of ſpirits; but how much cannot poſſibly be determined, without knowing the ſtrength of the ſpirits, and after what method it was graduated. At all events, the difference could ſcarcely have amounted to ten degrees, which would ſtill give the temperature of the air at leaſt 20° above the freezing point of quickſilver. Sal ammoniac with melting ſnow produces 32 degrees of cold; may we ſuppoſe, that it has a power of increaſing the cold nearly as much when it and the ſnow are previouſly cooled below that point *? But if ſo, why was not the quickſilver congealed till after a period of ſeveral hours, ſince other frigorific mixtures begin to act almoſt immediately? Beſides, there was not here the appearance of action, which conſiſts in a ſolution of the ſnow, inſtead of its freezing into a maſs. Or had the cold been greater where the mercury was placed than at M. BLUMENBACH's thermometer? The whole experiment remains

* See Biſhop WATSON's Chemical Eſſays, vol. III. p. 138.

involved in ſuch obſcurity, that ſome perſons have ſuppoſed the quickſilver itſelf was not frozen, but only covered over with ice, to which opinion, however, there are great objections.

It is worthy of remark, that Gottingen, though ſituated in the ſame latitude as London, and enjoying a temperate climate in general, becomes ſubject at times to a great ſeverity of cold. This of the 11th of January, 1774, is one inſtance. I find others when the thermometer ſunk there to $-12°$, $-16°$, or $-19°$; and at Cattlenburg, a ſmall town about two German miles diſtant, to $-30°$ *. By watching ſuch extraordinary occaſions, experiments on the freezing of quickſilver might eaſily be performed in many places where the poſſibility of them is at preſent little ſuſpected. The cold obſerved at Glaſgow in 1780 would have been fully ſufficient for that purpoſe †.

Dr. BLUMENBACH's deſcription of the ſolid quickſilver differs ſo much from Profeſſor BRAUN's, with reſpect to its colour and general appearance, as to require a particular explanation. Their diſagreement, I imagine, was occaſioned by a diverſity in the circumſtances of their experiments. Quickſilver cryſtallizes in becoming ſolid. In this property it reſembles other metallic ſubſtances, as appears from many facts, and is elegantly exemplified in thoſe curious cups which are formed by expoſing proper maſſes of melted metal to the cold air till the outer part be ſufficiently hardened to conſtitute a ſolid coat, and then letting out the internal fluid part, ſo as to leave a hollow in the middle. This concavity is found every where beſet with metallic cryſtals, ſcarcely yielding in beauty and regularity to

* LAXMANN's Sibiriſche Briefe, p. 98. Nov. Commentar. Petrop. tom. VII. p. 396.

† Viz. $-23°$ on the ſnow, $-14°$ in the air. Phil. Tranſ. vol. LXX. p. 456.

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the fineſt configurations of ſalts. In like manner, with regard to quickſilver, Profeſſor BRAUN himſelf obſerved, that whenever it had congealed but imperfectly, and the fluid part was poured off, the ſolid ſurface which came in view was extremely rough, as if compoſed of many ſmall globules. One of Mr. HUTCHINS's late obſervations exceedingly illuſtrates this matter; for he remarks, that when the fluid mercury was decanted off, in his tenth experiment, "the internal ſurface of the frozen "quickſilver ſhewed very uneven, with many radii going acroſs, "ſome of which had heads reſembling pins." Now in Profeſſor BLUMENBACH's experiment, the quickſilver lying looſe, except the flat ſide that touched the glaſs, could cryſtallize without impediment, and hence aſſumed a rough, and conſequently a dead-white ſurface; whereas in thoſe made by Mr. BRAUN, with tubes and thermometers, the metal being ſo much confined by the ſmooth glaſs, its ſurface was rendered of a high poliſh, not diſtinguiſhable in point of ſplendor from that of fluid mercury. Perhaps alſo, M. BLUMENBACH's quickſilver might have been made to look duller by ſome dirt or moiſture collected upon it from the ſal ammoniac and ſnow.

§ 3. In the notification of Profeſſor BRAUN's experiments by the French Academy of Sciences, a ſort of requeſt is inſerted, that proper perſons might be ſent by the Ruſſians into Siberia, and by the Engliſh to Hudſon's Bay, for the purpoſe of repeating them with every advantage of natural cold. I do not find that the Ruſſians have taken any notice of this, and indeed with regard to them it would be almoſt ſuperfluous, as ſcarcely a winter paſſes at Peterſburg in which the weather is not ſevere enough for the ready congelation of mercury by artificial means. But in this country, the Royal Society, ever attentive

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to the improvement of ſcience, deſired their late ſecretary Dr. MATY to make the neceſſary application to the Hudſon's Bay Company; in conſequence of which Mr. HUTCHINS, who was then in London, and going out with a commiſſion as governor at Albany Fort, offered to undertake the experiments, and executed them in a very complete manner, as appears from the account publiſhed in the LXVIth volume of the Philoſophical Tranſactions. In the months of January and February 1775, he twice froze quickſilver at Albany Fort; and in the firſt of theſe experiments, having broken his thermometer, he found, that the metal flattened by a fall of about ſix inches, bore to be hammered, gave a "deadiſh" ſound like lead, and was finely poliſhed on the ſurface. As Mr. HUTCHINS adopted exactly the method of Profeſſor BRAUN, he obſerved the ſame phænomena, encountered the ſame difficulties from the ſticking of the quickſilver in the tube, and cracking of his thermometer, and was equally at a loſs with regard to the point of congelation. Still, however, this was the fulleſt confirmation that M. BRAUN's Diſſertations had ever yet received; and it may be conſidered as a prelude, by which Mr. HUTCHINS acquired the experience that enabled him to ſucceed ſo perfectly in his laſt moſt deciſive and ſatisfactory experiments.

§ 4. The account of Mr. HUTCHINS's ſucceſs at Hudſon's Bay was read before the Royal Society at the commencement of the ſevereſt winter that had been known for many years in Europe. Two gentlemen of different countries embraced this opportunity to attempt the congelation of quickſilver. The firſt was Dr. LAMBERT BICKER, Secretary to the Batavian Society at Rotterdam, who, on the 28th of January, 1776, at eight in the morning, made an experiment to try how low he

could reduce the thermometer by artificial cold, the temperature of the air being then + 2°. He could not bring the mercury lower than − 94°, at which point it ſtood immoveable; and upon breaking the bulb he ſaw with certainty that the outer part of the quickſilver had loſt its fluidity, and was thickened to the conſiſtence of an amalgam; it fell out of the bulb in little bits, which bore to be flattened by preſſure, without running into globules like the inner fluid part.

Next day, when the thermometer ſtood at + 8°, he repeated the experiment with all poſſible exactneſs, after M. BRAUN'S manner; but could not obtain a greater deſcent of the mercury than to 80° under 0, and did not again break his thermometer.

The firſt account I ſaw of theſe experiments was in an extract from the Altona Poſt *, in the German language, which Sir JOSEPH BANKS had the goodneſs to procure from Gottingen at my requeſt; but as they are confirmed by Profeſſor VAN SWINDEN in his Obſervations on the cold of 1776 †, there can can be no doubt of their authenticy; and they afford a proof that the freezing point of quickſilver cannot be lower than − 94°, as it ſunk only to that degree, and yet was in part congealed.

§ 5. The other gentleman who tried the effect of this ſevere cold in 1776 upon mercury was Dr. ANTHONY FOTHERGILL, at Northampton, and the account of his experiment may be ſeen in the Philoſophical Tranſactions for that year ‖. His

* Altonaër Reichspostreuter, 1776. N° 24. Feb. 24.

† Obſervations ſur le froid rigoureux du Mois de Janvier, 1776, p. 277.

‖ Vol. LXVI. p. 589.

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frigorific mixture appears to have been made with the vitriolic acid; and the natural cold of the air at Northampton that day, the 30th of January, was +9°. It is ſcarcely poſſible to determine how far he ſucceeded. The quickſilver of his thermometer ſunk into the bulb, and it, as well as ſome in a phial, contracted what Dr. FOTHERGILL calls a film on the top; but unleſs the ſcale of his inſtrument went below −40°, or ſome ſolid cryſtals were formed, ſuch as M. BRAUN and others obſerved at the commencement of congelation, nothing can be collected with certainty from this experiment.

Though the cold of this hard winter was not ſufficient, either in England or Holland, for the convenient performance of experiments on the congelation of quickſilver, yet in many parts of the European continent, not farther north, it was fully intenſe enough for that purpoſe. The very morning when Dr. BICKER ſucceeded ſo imperfectly at Rotterdam, the thermometer ſunk to −22° at Rudolſtadt, ſituated a degree more to the ſouthward; and it had been the preceding day as low as −18° at Berlin *.

§ 6. I find no further attempts to freeze quickſilver till the year 1781, when Mr. HUTCHINS reſumed this ſubject with ſuch brilliant ſucceſs. The preceding experiments had done little more than prove that quickſilver might be rendered ſolid by cold, and ſhew what ſort of ſubſtance it was in that ſtate. Nothing ſatisfactory had been aſcertained with regard to its freezing point, or the degree of a thermometer at which it ceaſes to be a melted and becomes a ſolid metal. It muſt not be ſuppoſed, however, that the gentlemen who were engaged

* Beſchäftigungen der Berliniſchen Geſellſchaft Naturforſch. Freunde. B. II. p. 575, 576.

in theſe reſearches neglected ſuch a principal object of enquiry; on the contrary, Profeſſor BRAUN himſelf, as hath been already mentioned, took infinite pains to inveſtigate it, but, for want of perceiving the conſequences of the metal's great contraction in becoming ſolid, went very wide of the truth. This ſource of error did not eſcape the penetration of other philoſophers, ſeveral of whom declared their opinion that the degree of cold neceſſary for the congelation of quickſilver could hardly be determined by freezing a thermometer filled with that fluid. But Mr. CAVENDISH and Dr. BLACK were the gentlemen who ſuggeſted an adequate method of obviating the difficulty, ſo as to aſcertain the point in queſtion with certainty and preciſion. Reaſoning on the well-known fact, that a quantity of water continues at the ſame temperature from the moment it begins to freeze till the whole is become ſolid, they very juſtly concluded that the ſame would hold good with regard to quickſilver; and Mr. CAVENDISH confirmed this inference by experiments with metals of eaſy fuſion, in which he found a thermometer keep at the ſame degree all the time they were paſſing from a fluid to a ſolid ſtate. Hence it was propoſed, that a ſmall thermometer ſhould be placed in ſome quickſilver to be frozen; which ſinking pretty regularly till the congelation began, and remaining ſtationary till it ſhould be complete, would thus ſhew the degree of cold at which this effect takes place.

It is not at all ſurpriſing, that both the above-mentioned gentlemen ſhould have ſuggeſted the ſame method, without the leaſt communication of each other's ſentiments. Moſt diſcoveries follow ſo cloſely upon a certain ſtate of advancement in every ſcience, that, under the preſent general diffuſion of knowledge, ſimilar ideas muſt be expected to occur about the ſame time to ſuch perſons as are engaged in ſimilar purſuits. When the fruit is come to a certain degree of ripeneſs, more than one

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man may have ſtrength enough to ſhake it off. Were we ſure that philoſophy would continue to be regularly cultivated, perhaps it might with truth be affirmed, that the utmoſt efforts of genius amount only to a power of anticipating diſcoveries which would neceſſarily be made in the courſe of a few years by the common progreſs of mankind. The principles of this experiment for determining the point of congelation in mercury being already before the world, it was moſt probable, that the conſequences to be deduced from them would not eſcape gentlemen of ſuch acknowledged ſagacity, whenever they might happen to apply their attention to that ſubject.

Though the methods propoſed by Mr. CAVENDISH and Dr. BLACK were fundamentally the ſame, yet there was ſome difference in the apparatus they recommended; and as the former gentleman got *his* executed in London and ſent out to Hudſon's Bay, it was that which Mr. HUTCHINS employed in performing moſt of his experiments. A circumſtantial detail of theſe has ſo recently been read before the Society, that it would be tedious for me here to recapitulate the particulars. They have not only confirmed the preceding obſervations relative to the ſolid ſtate into which quickſilver can be brought by cold, its metalline ſplendor and poliſh when ſmooth, its roughneſs and cryſtallization where the ſurface was unconfined, its malleabillity, ſoftneſs, and dull ſound when ſtruck; but have alſo clearly demonſtrated, that its point of congelation is no lower than $-40°$, or rather $-39°$, of FAHRENHEIT's ſcale; that it will bear, however, to be cooled a few degrees below that point, to which it jumps up again on beginning to congeal; and that its rapid deſcent in a thermometer through many hundreds of degrees, when it has once paſt the above-mentioned limits, proceeds merely from its great contraction in the act of freezing. Theſe

and the other conſequences deducible from Mr. HUTCHINS's experiments have been ſo exactly pointed out by Mr. CAVENDISH, the real author and firſt mover of the whole buſineſs, that nothing remains for me but to add a few ſupplementary remarks.

Moſt of the appearances which perplexed M. BRAUN in his experiments admit of ſuch a ready ſolution from theſe of Mr. HUTCHINS, that it would be ſuperfluous to dwell upon each particular; there is one, however, which muſt be mentioned, becauſe an erroneous explanation has been given of it by a very eminent Swediſh philoſopher *. Profeſſor BRAUN obſerves, that by a certain kind of management he could effect the congelation of quickſilver with very weak aqua fortis. For this purpoſe he filled ſeveral different glaſſes with ſnow, into which he ſucceſſively poured the dilute acid of nitre, and immerſed his thermometer. The mercury, which would ſink only to $-148°$ in the firſt glaſs, came in the fourth to the term of complete congelation. It is no wonder that the profeſſor, with the ideas he entertained, ſhould think this extraordinary; but we now clearly underſtand, that the cold in the firſt glaſs was ſufficient to freeze part of the quickſilver, but did not laſt long enough to render the *whole* ſolid; in the ſecond glaſs a further part froze; in the third ſtill more; till at length only ſuch a quantity was left as the fourth mixture could fully congeal.

Another phænomenon, of which M. BRAUN gives a very unſatisfactory ſolution, is the ſinking of the quickſilver in his thermometers after they were taken out of the frigorific mixture. This, I ſuppoſe, proceeded entirely from its beginning to melt in the warmer air, and conſequently ſubſiding to fill up vacuities in the ſtem or ball, in the ſame manner as hap-

* Kongl. Vetenſk. Acad. Handlingar vol. XXXIII. p. 119.

pened very remarkably in ſome of the experiments at Hudſon's Bay, and in various meteorological obſervations to be deſcribed hereafter.

Mr. HUTCHINS mentions, in the remarks upon his ninth experiment, that the internal ſurface of the congealed quickſilver, after the ſuperincumbent frigorific mixture and fluid metal had been decanted off, "was every where very rough, and of a "dull white, reſembling that of a ſilver ſpoon in common "uſe;" likewiſe, that "the lump ſhewed very weak coheſion, "crumbling to pieces under the ſtrokes of a hammer, and had "not the uſual poliſh." Theſe differences he aſcribes to an effect produced, by the ſpirit of nitre in the mixture, upon the quickſilver, with which it came into immediate contact; but I am rather of opinion, that they were occaſioned chiefly by the unconfined cryſtallization of the mercury in the open gallipot employed for this experiment, ſimilar appearances having always been obſerved when it congealed under ſuch circumſtances.

It is here neceſſary to take notice, that the thermometers ſent out to Mr. HUTCHINS, even thoſe made with quickſilver, did not exactly agree together. From his *Table of Compariſon* we may collect, what ſubſequent experiments have confirmed, that $-40°$ upon the two ſmall ones with ivory ſcales, anſwered to $-44°$ or $-45°$ upon one of the large thermometers, that which had the ball on top, and to $-42°$ or $-43°$ upon the other. Now as this laſt was the inſtrument which Mr. HUTCHINS plunged into the quickſilver he attempted to freeze in his eighth experiment, and which ſunk only to $-40°$, it is no wonder that he did not then perceive any marks of congelation, the cold really wanting two or three degrees of being ſufficient for that effect.

The

The old ſpirit-thermometer called *H* was erroneous no leſs than 7° or 8° near the freezing point of water, which ſerved as a kind of compenſation in the lower parts of the ſcale, ſo as to make it agree tolerably well with thoſe of mercury in the greateſt cold of the frigorific mixtures.

Although in two of Mr. HUTCHINS's thermometers the quickſilver ſunk exceedingly low, to −450° or near −500°, there is reaſon to believe he did not in any inſtance obtain the extreme term of contraction, ſince Profeſſor BRAUN, in ſome of his laſt experiments, brought the mercury in one thermometer to −544°, and in another to −556° *. Hence it would ſeem, that Mr. HUTCHINS had always ſome part of the quickſilver left unfrozen, or ſome vacuity remaining, either in the ſtem or the ball of his inſtruments; and as no objection appears againſt thoſe experiments of M. BRAUN's, we muſt conclude, that quickſilver, in becoming ſolid, contracts about a 23d of its whole bulk. When the principal object in view is to determine the quantity of this contraction, it will be moſt expedient to perform the experiment in the leaſt degree of cold which will permit the quickſilver to be entirely frozen, that it may not be ſo likely to ſtick faſt in the tube; but care muſt be taken to congeal the whole of the mercury in the ſtem as well as that in the bulb.

Among the numerous improvements in natural knowledge which have been made within a ſhort period of years, perhaps none tends to illuſtrate more phænomena of nature than the late diſcovery, that a conſiderable quantity of heat diſappears when bodies paſs into a ſtate of fluidity or elaſtic vapour, and re-appears when they are converted back again to their original condition. This remarkable effect of ſuch changes, I believe,

* Nov. Comment. Petrop. tom. XI. p. 313.

was firſt obſerved at Glaſgow, about twenty years ago, by Dr. BLACK and Mr. IRWIN, who endeavoured to determine its moſt material circumſtances by various experiments. Since that time Dr. BLACK has conſtantly taught it in his chemical lectures; and conſidering the heat which diſappears as ſtill remaining in the fluid or vapour, but deprived for the time of its property of being communicated to other bodies, and thereby becoming ſenſible, he calls it *latent* heat, a term ſufficiently expreſſive of his manner of conceiving the fact.

In the year 1772, the celebrated Profeſſor WILCKE inſerted, in the Tranſactions of the Royal Academy of Sciences at Stockholm *, a paper profeſſedly on the ſubject of *the cold produced by ſnow in melting*, which being written in the Swediſh language is leſs known in this country than it deſerves. He ſeems not at all acquainted with what Dr. BLACK had done, but ſpeaks of it as his own diſcovery, originating in an accidental attempt to melt away a quantity of ſnow by the affuſion of hot water; when he found the proceſs go on ſo ſlowly, and ſo little effect produced, that he determined to inveſtigate the cauſe of ſo unexpected an event. After a ſeries of experiments with this view, he came to the following concluſion; that ſnow, in melting, conſtantly abſorbs a certain and equal quantity of heat, which is employed entirely in giving it fluidity. To render ſuch a property more intelligible, M. WILCKE propounds a particular theory of an elaſtic fluid between the particles of bodies; and he proceeds to various illuſtrations and deductions, all highly ingenious.

Two principal methods have been adopted to prove this loſs of heat; one, by adding ice at the freezing point to a certain proportion of water at a known degree of heat, and obſerving how much the temperature of the mixture comes out below

* Kongl. Vetenſkaps Acad. Handlingar vol. XXXIII. p. 97.

that

that which ſhould have reſulted according to the common laws of the diſtribution of heat among bodies; the other, by obſerving how much faſter water near the freezing point acquires ſenſible heat, than an equal quantity of ice melting under ſimilar circumſtances. It is obvious, that both theſe methods tend not only to prove the fact, but likewiſe to diſcover the quantity of heat ſo abſorbed; and that the latter alſo, if the operation be reverſed, will ſhew the quantity of heat evolved, when a fluid congeals or becomes ſolid. In this way Dr. BLACK eſtimates the heat in queſtion to be equal to 140 degrees upon FAHRENHEIT's ſcale; Profeſſor WILCKE, by a great variety of experiments with different proportions of ſnow and water, brought it out pretty uniformly about 130; and Mr. CAVENDISH, as he hath lately informed us, finds it amount to 150, and chooſes to call the proceſs a generation of heat or production of cold *.

As the method put in practice by Mr. HUTCHINS to ſettle the freezing point of quickſilver depends entirely upon this generation, re-appearance or evolution of heat, by means of which the congealing quickſilver is kept at the ſame temperature as long as any conſiderable portion of it remains fluid, I thought ſome account of ſuch an intereſting diſcovery would not here be miſplaced. It now becomes an important object of attention, in examining the properties of bodies, to inveſtigate the quantity of heat produced or loſt at their melting and vaporific points. So little, however, has hitherto been done in this reſpect, even with thoſe bodies that are changing from fluid to ſolid every day before our eyes, that it is no wonder we are yet unable to determine it in a ſubſtance which has ſo ſeldom been ſeen in a ſolid ſtate. But from the very quick congelation of quickſilver when placed in a frigorific mixture, as ſhewn by its

* Meſſ. LAVOISIER and DE PLACE, from ſome late experiments with their new apparatus, fix it at 135.

rapid

rapid deſcent in the thermometer, and from its readineſs to melt again upon an abatement of the cold, apparent in all the experiments, and particularly noticed by M. BRAUN, there is reaſon to believe, that the quantity of heat employed in giving it fluidity is not very conſiderable. When water, which has been cooled below + 32°, begins to freeze, a certain part of it, proportioned to the degree of cooling, ſhoots at once into ice; that is, ice continues to be formed till ſo much heat be evolved as is requiſite to bring the whole up again to + 32°. Now I am inclined to ſuſpect, that in ſeveral of Mr. HUTCHINS's experiments the firſt jump of the quickſilver down from a little below the point of mercurial congelation, depended on a ſimilar principle of the ſudden freezing of ſuch a proportion of the mercury as correſponded to the number of degrees it had been cooled below that point; hence, if the degree to which it bore to be cooled before it began to congeal, and the contraction it ſuffers in congealing, were both known, its quantity of latent heat, to ſpeak in Dr. BLACK's language, might readily be found. From a rude and vague computation of this ſort, I am led to believe, it is not half that of water; and if ſo, quickſilver ſeems to differ much in this reſpect from other metals; for tin is ſaid, from Mr. IRWIN's experiments, to require, in order to melt, a quantity of heat which, if ſet looſe and rendered ſenſible, would raiſe the thermometer 500 degrees.

Beſides the inſtruments contrived particularly to try the freezing point of quickſilver, two ſpirit-thermometers alſo were ſent out to Hudſon's Bay, principally with a view to ſome collateral circumſtances of the experiment. My intention in recommending them was to diſcover what degree of cold the freezing mixture produced; and to obtain a more exact compariſon of the relative contractions of mercury and alcohol, by

marking their ſimultaneous deſcents on a more extended ſcale, or as long as both of them ſhould continue to contract regularly. The ſpecific gravity of the alcohol employed to make theſe thermometers was found to be 0,8254, in a temperature of 58°½; and they were graduated on the principle of two fixed points, one, the real freezing point of water fixed by actual experiment; the other, the 122d degree above 0, determined by compariſon with a ſtandard mercurial thermometer. This interval being divided into 90 parts, the degrees ſo found were meaſured downward as well as upward on the ſcale, with a proper allowance for inequality in the bore of the tube. A ſubſequent experiment ſhewed that the freezing point had been rightly marked upon theſe inſtruments; but that, in conſequence of a common fault in conſtructing thermometers, of not heating the contents of the tube ſo much as thoſe of the ball, the point of 122° was marked on both of them lower than it ought by the ſpace of two degrees; ſo that 122° on the ſcale indicated only 120 degrees above 0 of real heat. Any detail of the obſervations that were made to ſettle the relative contractions of quickſilver and ſpirits by means of theſe inſtruments would be improper at preſent; it is ſufficient to mention, that on one of them the 29th degree below 0, and on the other the 30th, were found to correſpond with −40° of the ſmall mercurial thermometers, or more preciſely with the point that would have been −39° upon an exact ſtandard inſtrument.

The other object for which the ſpirit-thermometers were propoſed, is more immediately connected with the congelation of mercury. All former experiments with frigorific mixtures had left us abſolutely in the dark with regard to the degree of cold that was really produced. By theſe inſtruments it is now determined, that the greateſt effect of a mixture of

ſnow

ſnow and ſmoaking acid of nitre, even with the advantage of ſuch natural cold as congealed the quickſilver expoſed to it, was only to diminiſh the heat to ſuch a degree as would correſpond with − 45° or − 46° of a ſtandard mercurial thermometer; and conſequently that the cold obtained by FAHRENHEIT in the firſt experiments with ſuch mixtures, which BOERHAAVE ſtates at − 40°*, cannot be exceeded but by a very few degrees. This reſult is the more ſurpriſing, on account of BRAUN's poſitive aſſertions, that his thermometer both of rectified ſpirits and eſſential oils deſcended 150 or at leaſt 100 degrees below 0 †. But, ſince that gentleman was ſtrongly impreſſed with an opinion of the exceſſive cold neceſſary to freeze quickſilver, ſo much as to ſhew evident perplexity at finding his ſpirit-thermometers ſink leſs than thoſe of mercury, I ſhould, from this circumſtance alone, be inclined to place moſt confidence in the experiments made at Hudſon's Bay, in which no hypotheſis was adopted, and therefore no prejudice can be apprehended. Indeed Mr. HUTCHINS's obſervations with regard to the degree of cold generated by his freezing mixtures, are ſo regular, uniform, and numerous, as hardly to leave a doubt that it does not exceed − 35° or − 36° of his ſpirit-thermometers. And this is, of itſelf, a very great additional proof, that the freezing point of quickſilver cannot be much lower than Mr. HUTCHINS determines it, ſince the mixture was incapable of diminiſhing the heat more than ſix or ſeven degrees further. The advantages ariſing from a knowledge of the cold produced, were ſo apparent in theſe experiments at Hudſon's Bay, with reſpect to many circumſtances, both in the congelation of the quickſilver itſelf,

* BOERHAAV. Element. Chemiæ, tom. I. p. 164.

† Nov. Comment. Petrop. tom. XI. p. 290. 316. 317. From 260° to 300° of DE L' ISLE's ſcale.

and in the action of the frigorific mixtures, that I ſhould ſuppoſe ſpirit-thermometers will always be employed in future, whenever any thing of this kind is attempted.

§ 7. It is not a little extraordinary, that ſince the death of Profeſſor BRAUN, now near fifteen years ago, all attention to the congelation of mercury ſhould in a manner be laid aſide on the ſpot where it was originally diſcovered. A dead ſilence on the ſubject ſeems to have prevailed at Peterſburgh till this preſent winter; when Dr. MAT. GUTHRIE, F. R. S. Phyſician to the Cadet Corps of Nobles, having heard the matter much canvaſſed during his late viſit here, reſumed the conſideration of it on his return to that metropolis. The only intelligence I have yet received of Dr. GUTHRIE's experiments or concluſions, is contained in one of his letters to Dr. GARTHSHORE of this Society, who has obligingly favoured me with the following extract.

"Having found," ſays Dr. GUTHRIE, "in my late journey "to Britain, that it remained a matter of doubt, whether mer-"cury in its pure ſtate, unmixed with heterogeneous matter, "had ever been or was capable of being congealed; I am glad "to be able, from the reſult of ſeveral experiments, to inform "you, that the pureſt mercury known to the chemiſts is capa-"ble of congelation, and in that ſtate will bear the hammer.

"I have done ſomething alſo toward determining the point of "its congelation, by determining what it is not, *viz.* 150° of "REAUMUR's thermometer.

"A fine thermometer, made by NAIRNE, graduated 150° "of REAUMUR, that is, 337° of FAHRENHEIT below the "freezing point" [or −305°] "ſunk entirely into the bulb, "while the mercury in which it was plunged remained per-"fectly liquid, nay had not as yet grown thick and gritty, a

"phenomenon

" phenomenon that always precedes congelation, as I have " found in my experiments; nor had there as yet been formed " in the inſide of the tube containing the mercury to be frozen " (and the thermometer to determine the point of congelation " with which I ſtirred it) an incruſtation of the metal, another " indication of approaching congelation, which ever begins on " the ſide of the tube, and gradually increaſes till it has " reached the center, and a ſolid cylinder is produced.

" From this you may form a judgement of the impurity of " the mercury which ſome pretend to have ſeen congealed with " natural cold; for here 150° of REAUMUR was not found a ſuf- " ficient degree of cold to freeze it, and ſurely no ſuch abſence " of heat, or any thing near it, has ever been, or ever could " be, obſerved on the face of the habitable globe.

" I ſhall only add, that my experiments were conducted on " the plan of my learned friend Dr. BLACK, and ſpiritus nitri " fumans Glauberi with ſnow, were employed to produce an " artificial cold, while the thermometer of REAUMUR ſtood at " 20° below 0 in the open air" [that is, − 13° of FAHREN- HEIT's ſcale.]

Though the conſequences here deduced by Dr. GUTHRIE from his experiment are undoubtedly erroneous, as appears from a ſufficient number of other facts, yet it is not at all ſurprizing that they ſhould have ſeemed to him juſt; for the error ariſes from a circumſtance, which could not be foreſeen with certainty, and occurred in ſeveral of Mr. HUTCHINS's experiments as well as in Dr. GUTHRIE's. To underſtand this, it muſt be conſidered, that when we attempt to aſcertain the freezing point of water, by keeping a thermometer immerſed in it while it is changing into ice, the inſtrument employed for this purpoſe is not made of water, but of a different fluid, not ſubject to be peculiarly

affected

affected by that particular degree of cold. In order, therefore, to render the experiment with quickſilver perfectly analogous, it would be neceſſary not to make uſe of a mercurial thermometer; but to ſubſtitute ſuch a one as is capable of ſuſtaining a greater intenſity of cold. For otherwiſe, if it ſhould happen, from any circumſtance, that the quickſilver in the thermometer ſhould begin to freeze before that in which it is plunged, the whole experiment muſt evidently be fruitleſs, as the former would ſink, perhaps, many hundreds of degrees in the inſtrument, by its own contraction in becoming ſolid, while the ſurrounding mercury ſtill retained its fluidity. Now this was preciſely the caſe in Dr. GUTHRIE's experiment; the thermometer, with which he ſtirred his quickſilver, congealed, it would ſeem from the great deſcent, almoſt entirely, though he could not perceive in the quickſilver ſo agitated the leaſt appearance of change to a ſolid ſtate. Thus, likewiſe, in ſeveral of the experiments at Hudſon's Bay, the mercury in the encloſed thermometer was found to freeze before that in the cylinder. Hence it is manifeſt, that the continuance of fluidity in a quantity of quickſilver does not ſecure a thermometer of that metal immerſed in it from freezing.

The cauſe of this phenomenon is extremely uncertain. Poſſibly the point of congelation may not be exactly the ſame in all quickſilver under all circumſtances. Foreign admixtures may occaſion a difference in this reſpect; and it does not follow, that the effect of ſuch, in certain proportions, muſt neceſſarily be to make the mercury congeal ſooner, ſince, in the caſe of the fuſible metal, the melting point of tin is brought lower by the addition of two metallic ſubſtances, both of which ſeparately require a ſtronger heat than it for their fuſion.

But as quickſilver bears to be cooled ſome degrees below its freezing point, before it begins to form ſolid cryſtals, the phe-

nomenon in queſtion may depend upon that circumſtance: for if, from whatever cauſe, the mercury in the thermometer ſhould begin to congeal as ſoon as it was cooled down to $-39°$ or $-40°$, whilſt that which ſurrounded it would ſuſtain a cold of $-43°$ or $-44°$ without becoming ſolid; it is evident, that the whole of the former might be congealed, and yet no part of the latter, though the real freezing point of both were the ſame, that is, though the ſurrounding quickſilver as ſoon as it came to ſhoot its cryſtals would riſe immediately to $-39°$, the point at which that in the thermometer froze.

As this is undoubtedly the moſt obſcure part of our knowledge relative to the congelation of quickſilver, I endeavoured to illuſtrate it by ſome experiments on the freezing of water. The pureſt water I could obtain bore to be cooled to $+21°$, no leſs than eleven degrees below the temperature to which it inſtantly roſe as ſoon as the cryſtals of ice ſhot through it. This was diſtilled water very recently boiled; it is a miſtake, therefore, that boiling neceſſarily renders water not ſo capable of being cooled below the freezing point. In proportion as the water was leſs pure, it ſeemed to congeal the ſooner; and the kind of impurity which had the moſt effect appeared rather to be extraneous matter diffuſed through the water, ſo as to trouble its tranſparency, than ſuch as was chemically diſſolved in it *. The ſmalleſt particle of ice, alſo, whenever the water was below the freezing point, either added from without, or by any means formed in it, would inſtantly cauſe a cryſtallization, by which the whole came immediately up to $+32°$. Likewiſe a crack in the bottom of the containing glaſs veſſel

* I take this to be the reaſon that boiling has been thought to render water incapable of being cooled below the freezing point. In moſt kinds of water, the application of heat occaſions the precipitation of earthy ſubſtances which were before held in ſolution; hence the water comes to be in the ſtate of having extraneous matter diffuſed through it, and therefore readily congeals.

effectually

effectually prevented the water from being cooled below the freezing point, as ice conſtantly formed on the bottom, perhaps in conſequence of the early generation of ſome minute portions of it in the crack. But independently of theſe circumſtances, neither ſtirring, agitation, a current of freſh air on the ſurface, nor the contact of any extraneous body not colder, would cauſe the water to ſhoot into ice, even after it was cooled many degrees below the freezing point, notwithſtanding the repeated aſſertions of authors to the contrary.

How far theſe facts may be applicable to the above mentioned inſtances, where the thermometer froze before the quickſilver in which it was immerſed, can ſcarcely be determined unleſs more particulars were known. They ſhew, however, that the congelation would not neceſſarily be brought on by ſtirring the quickſilver, as practiſed by Dr. GUTHRIE; and point the way to various conjectures upon this difficult phenomenon, the diſcuſſion of which muſt be reſerved for a future opportunity.

This ſource of error in the method for ſettling the point of mercurial congelation, may eaſily be obviated by a ſmall change in the apparatus. Nothing further is neceſſary than to employ thermometers made of alcohol, eſſential oils, or ſuch other fluids as will bear the requiſite cold without freezing. Probably the former of theſe will be found moſt convenient: and although the contraction of other fluids does not exactly keep pace with that of quickſilver, yet as the relative proportions can be readily determined, experiments with them may at all times be reduced to the mercurial ſtandard, being not only the moſt familiar, but likewiſe that which ſeems to correſpond beſt with equal increments and decrements of heat.

There is one way alſo in which mercurial thermometers may be employed to aſcertain the freezing point of quickſilver; I mean by

by plunging them into ſome of that metal which has been frozen and is now melting. This was put in practice very ſucceſsfully by Mr. HUTCHINS in his ninth and tenth experiments. It anſwers to the method of determining the common point of congelation upon a thermometer by melting ice, well known to be more ſteady and certain in its temperature than freezing water. If, however, the point of mercurial congelation be not exactly the ſame in different portions of the metal, it is evident, that no reliance could be placed on ſuch an experiment; and it can ſcarcely be executed but with the greateſt advantage of natural cold.

As Dr. GUTHRIE was miſtaken in ſuppoſing he had proved that quickſilver did not congeal till it was cooled under −505°, his ſuggeſtion of impurity in the mercury employed by others is clearly without foundation. The inſtances to which he refers, when that metal froze by the natural cold of the air, are rendered certain and unexceptionable, from a great variety of concomitant circumſtances, confirmed by the moſt credible teſtimony, as ſhall preſently be ſhewn *.

§ 8. This account of mercurial congelation by artificial means would remain incomplete, were I not to mention that at Hampſtead, on the 26th of February laſt, the temperature of the air being then above +20°, Mr. CAVENDISH, by an ingenious artifice of diluting the nitrous acid to a proper degree,

* Since this was written, Dr. GUTHRIE has ſent a more perfect account of his experiments. They agree in the main with Mr. HUTCHINS's, and the difficulties which occurred to him may be ſolved on the ſame principles. It ſeems not improbable, that the thermometer with which the Doctor ſtirred his quickſilver had, by ſome accident, in the courſe of a long experiment, come into contact with the frigorific mixture, and ſo been ſet freezing.

ſunk the quickſilver in his thermometer to 110°, and conſequently froze it in part. He then interrupted the experiment to try the cold of his frigorific mixture by a ſpirit thermometer, and found it nearly as great as Mr. HUTCHINS had ever produced at Hudſon's Bay, that is, about equal to −45° of a ſtandard mercurial thermometer.

PART II.

NO other experiments have been inſtituted, as far as hath come to my knowledge, for the purpoſe of rendering quickſilver ſolid by frigorific mixtures; therefore I now proceed to a new ſeries of facts, which ſerve partly to confirm the former, and partly to ſhew their application. Though the congelation of mercury, abſtractedly conſidered, muſt be allowed to form a very curious and important epocha in the hiſtory of that metal, yet it is as having a reference to thermometers, by teaching us what dependence can be placed upon thoſe inſtruments, fixing our ideas with regard to the different diminutions of heat, and enabling us to form a juſter eſtimate of climates, that it chiefly becomes intereſting to the human race. The ſubſequent part of this narrative will demonſtrate, that quickſilver has very frequently become ſolid by natural cold; that in a few inſtances the effect was ſo palpable and obvious as to ſtrike with immediate conviction; but that in moſt it has never been even ſuſpected till the preſent time, the ſtrange appearances which often occurred being imputed by the obſervers to

any other rather than the real cauſe, though they are now found to carry with them a force of internal evidence which eſtabliſhes the truth beyond all doubt.

In enumerating theſe facts, I ſhall continue to purſue a chronological order. They are in general of ſuch a kind as could ſcarcely become an object of attention, till thermometers had acquired ſome degree of accuracy. This did not happen till near the year 1730, and the firſt obſervations which prove the freezing of quickſilver were made within four or five years of that period: ſo intimately are improvements in philoſophy connected with the perfection of inſtruments!

§ 1. When the Empreſs ANNA IWANOVNA had aſcended the throne of Ruſſia, ſhe reſolved to carry into execution one of the favourite ideas of her illuſtrious uncle, PETER the Great, by ſending out proper perſons to explore and deſcribe the different parts of her vaſt dominions, and enquire into the communication between Aſia and America. Three profeſſors of the Imperial Academy were choſen for this expedition; Dr. JOHN GEORGE GMELIN, in the department of Natural Hiſtory and Chemiſtry; M. GERARD FREDERIC MULLER, as general Hiſtoriographer; and M. LOUIS DE L'ISLE DE LA CROYERE, for the department of Aſtronomy; draughtſmen and other proper aſſiſtants were appointed to attend them. In the ſummer of the year 1733 they departed from Peterſburg; and though a principal object of their commiſſion was unavoidably neglected, from the difficulty of tranſporting the neceſſary ſupplies of proviſions to Kamchatka, yet it was the tenth year of their travels before the ſurvivors returned to Europe.

The thermometrical obſervations made in the courſe of this memorable ſurvey of the Ruſſian empire were communicated

to the world by Profeſſor GMELIN. His firſt account of them appeared in the preface to his Flora Sibirica *, where a few of the moſt remarkable are adduced as proofs of the exceſſive rigour of the Siberian climate; but they were afterwards given at full length, with a more ſatisfactory detail of circumſtances, in M. GMELIN's journal of his travels †, publiſhed by himſelf ſome years after his return. An abſtract of them was alſo inſerted in the Peterſburg Commentaries for the years 1756 ‡ and 1765 §, taken, after the profeſſor's death, from his original diſpatches, in poſſeſſion of the Imperial Academy. All the accounts agree together tolerably well; but as the journal is more immediately from the author, in his native language the German, and commonly contains moſt particulars, I thought it right to adhere principally to that work in the following narration of the facts ||.

It was at Yeniſeiſk, lat. 58° ½ N. and long. 92° E. of Greenwich, that M. GMELIN firſt obſerved ſuch a deſcent of his thermometer as, we now know, indicated the mercury to have been frozen. This happened in the winter of 1734 and 1735. "Here," ſays the profeſſor ¶, "we firſt experienced the truth "of what various travellers have related, with reſpect to the "extreme cold of Siberia; for, about the middle of December, "ſuch ſevere weather ſet in, as, we are certain, had never been "known in our time at Peterſburg. The air ſeemed as if "it were frozen, with the appearance of a fog, which did not

* P. lxxi—lxxiii.

† Reiſe durch Sibirien.

‡ Nov. Comment. Petrop. tom. VI. p. 425.

§ Ibid. tom. XI. p. 320.

‖ See alſo Mem. de l'Acad. des Sciences, 1749, p. 1.

¶ Reiſe, Theil. I. p. 355.

"ſuffer

"ſuffer the ſmoke to aſcend as it iſſued from the chimnies. "Birds fell down out of the air as if dead, and froze immediately, unleſs they were brought into a warm room. "Whenever the door was opened, a fog ſuddenly formed round it. "During the day, ſhort as it was, parhelia and haloes round the "ſun were frequently ſeen, and in the night mock moons and "haloes about the moon. Finally, our thermometer, not ſubject to the ſame deception as the ſenſes, left us no doubt of "the exceſſive cold; for the quickſilver in it was reduced" [on the 5th of Jan. O. S.] "to −120° of FAHRENHEIT's ſcale, "lower than it had ever hitherto been obſerved in nature."

Thus far Profeſſor GMELIN. Little did he conceive that, though his thermometer was not ſubject to the ſame deception as the ſenſes, it lay expoſed to another ſource of error which defeated all his concluſions: for as ſoon as the cold became ſufficiently great to produce any congelation of the quickſilver, it ceaſed to be a meaſure of the temperature; inſtead, therefore, of 120° below 0, the cold moſt probably did not exceed the point of mercurial congelation, or −39°, but by a very few degrees, the great deſcent of the quickſilver, as it depended upon its contraction in the act of freezing, only affording a proof that it had really ſuffered this change.

We muſt here obſerve, that Dr. GMELIN's thermometers were conſtructed by M. JOSEPH NICHOLAS DE L'ISLE, brother to the gentleman who went upon this expedition, on the principle invented by himſelf, and which ſtill bears his name. At preſent ſuch thermometers are always made by determining two fixed points, of which the uppermoſt, or that of boiling water, is aſſumed as 0, and the lowermoſt, or the point of melting ice, as 150°, the ſcale being counted downward; but in their original conſtruction, when the utility of fixed points was

was leſs underſtood, M. DE L'ISLE took the degrees of his ſcale from decrements in the bulk of the quickſilver equal to ten thouſandth parts of its whole volume at the heat of boiling water. By this method the freezing point ſeems to have fallen about the 152d degree *; and accordingly Profeſſor GMELIN, whenever he has occaſion to expreſs his obſervations in the numbers of FAHRENHEIT's ſcale, reduces them on that ſuppoſition. It is not eaſy to diſcover the exact time when the preſent method of reckoning M. DE L'ISLE's degrees commenced; but ſo early as in M. BRAUN's experiments it is expreſſly ſtated, that the freezing point of his thermometer was only 150° †. There can be no doubt, however, both from theory and from WEITBRECHT's ‡ and GMELIN's obſervations, that in the thermometers uſed during this Siberian journey, the degree at which water congealed was nearly as low as 152°; it is according to this proportion, therefore, that I ſhall compute all Dr. GMELIN's obſervations, adhering to the common rule for ſuch as have been made ſubſequent to his time with DE L'ISLE's thermometer.

The next inſtance of mercurial congelation to be found in GMELIN's journal exhibits a very ſtriking example of the force of prejudice. It happened at Yakutſk, lat. 62° N. and long. 130° E. in the winter of 1736 and 1737, and is thus related by the profeſſor §. "This winter was unuſually mild here, "nevertheleſs we endured at times very ſevere cold, being froſt-"bitten in a ſledge within the ſpace of ſix minutes, notwith-"ſtanding all our precautions. One day, alſo, a certain per-

* GMELIN's Reiſe. Theil. III. p. 143.

† Nov. Comment. Petrop. tom. XI. p. 299.

‡ Ibid. tom. X. p. 303.

§ Reiſe. Theil. II. p. 451.

"ſon,

" ſon, who has ſome reputation in the learned world on ac-
" count of his obſervations in natural philoſophy, informed
" me by a note, that the quickſilver in his barometer was fro-
" zen. I haſtened immediately to his houſe, to ſee this hitherto
" incredible wonder of nature. Not feeling by the way the
" ſame effects of cold as I had experienced at other times in
" leſs diſtances, I began, before my arrival, to entertain ſuſpi-
" cions about the congelation of his quickſilver. In fact, I
" ſaw that it did not continue in one column, but was divided
" in different places as into little cylinders which appeared
" frozen, and in ſome of theſe diviſions between the quick-
" ſilver I perceived an appearance like frozen moiſture. It im-
" mediately occurred to me, that the mercury might have been
" cleaned with vinegar and ſalt, and not ſufficiently dried. The
" perſon acknowledged it had been purified in that manner.
" This ſame quickſilver, taken out of the barometer and well-
" dried, would not freeze again, though expoſed to a much
" greater degree of cold, as ſhewn by the thermometer. We
" were aſſured by the inhabitants, that the ſevereſt cold of this
" winter did not approach what they had ſuffered other years;
" and yet the thermometer fell ſeveral times to 72° below o of
" FAHRENHEIT's ſcale, which would be thought, in Germany
" at leaſt, a very intenſe froſt."

The gentleman to whoſe obſervation Dr. GMELIN here ſhews ſo little reſpect, ſeems to have been no other than one of his aſſociates in the commiſſion, M. DE L'ISLE DE LA CROYERE*, probably the firſt perſon upon earth who ſaw quickſilver reduced to a ſolid form by cold, and ventured to credit the teſtimony of his ſenſes. As to the objection, that the ſame mercury did not freeze with a greater degree of cold, it is of no avail; for M.

* See Dr. HIMSEL's Letter, Phil. Tranſact. vol. LI. p. 673.

GMELIN.

GMELIN had not any other means of eſtimating this but by the deſcent of his thermometer, which could be depended upon no farther than to the point of mercurial congelation. Nay, it is not improbable, that the more violent the cold, the leſs would the quickſilver appear to ſink below that point, from the quicker freezing and adheſion of the ſmall thread of mercury in the thermometrical tube. Beſides, a part of the quickſilver expoſed to the air might eaſily be frozen, and yet no appearance of ſuch a change be perceived, if the maſs did not any where ſeparate or divide. And the fact, that it actually did freeze ſeveral times during the winter is put beyond all doubt, by the ſinking of the thermometer ſo many degrees below the term at which that effect begins to take place. The abſurd idea, that quickſilver appears to congeal in conſequence of water it contains, was derived, I believe, originally from a whim of RAYMOND LULLY's. It has been the uſual refuge of thoſe gentlemen who thought proper to deny that mercury could be made ſolid by cold; but is too deſtitute of ſupport to merit confutation.

Profeſſor BRAUN mentions, on two different occaſions *, that the Peterſburg Academy have in their poſſeſſion ſome obſervations made in Siberia, which ſeem to ſhew the congelation of mercury by natural cold; but that little credit was given to them, becauſe it had at other times been found to retain its fluidity when the cold was much more intenſe. Probably the obſervations here meant are M. DE L'ISLE DE LA CROYERE's; for that gentleman certainly tranſmitted papers to the Academy, according to which the mercury became ſolid as ſoon as it fell about 200° below 0 of his brother's thermometer †, cor-

* Nov. Comment. Petrop. tom. VIII. p. 363. and tom. XI. p. 269.

† Phil. Tranſact. vol. LI. p. 673.

reſponding

reſponding with − 25° of FAHRENHEIT's ſcale. This eſtimation, though now found to be many degrees leſs than the truth, yet approaches it ſo near as to impreſs a very favourable idea both of M. DE L'ISLE's talent for obſervation, and of his ſuperiority to vulgar prejudices. It is to this ſame gentleman that we are indebted for an account of the aſtoniſhing ſeverity of the climate in the north-eaſternmoſt extremities of Aſia. Yakutſk itſelf, lying further in that direction than any other place where M. GMELIN reſided, evidently partakes of the ſame rigorous cold, if a winter in which quickſilver froze ſeveral times was eſteemed unuſually mild by the inhabitants.

Another ſet of obſervations, in the courſe of which the mercury frequently congealed, were made by Profeſſor GMELIN at Kirenga Fort, lat. 57½ N. long. 108 E. in the winter of 1737 and 1738. His thermometer on different days ſtood at − 108°, − 86°, − 102°, − 113°, and ſeveral intermediate degrees. Some extraordinary appearances, which very much perplexed him in theſe obſervations, not only admit of a ready ſolution from Mr. HUTCHINS's determination of the freezing-point of quickſilver, but alſo confirm it with wonderful preciſion.

On the 27th of November (O. S.) after the thermometer had been ſtanding two days at − 46°, the profeſſor found it ſunk at noon to 108°. He adds*: "I had ſcarcely noted down "this obſervation, when ſuſpecting ſome miſtake, I ran back "and examined it again. I ſaw the quickſilver now at 102°, "and it continued riſing ſo faſt, that in the ſpace of half an "hour it had reached to − 19°." The explanation of this phænomenon, which appeared ſo odd to M. GMELIN, is very evident. When the intenſe cold ſet in, the quickſilver froze in

* Reiſe. Theil. II. p. 619.

his thermometer, and ſtuck in the tube at −46°, that is, a few degrees below the true point of mercurial congelation. But the weather becoming milder two days afterwards, the ſmall thread of quickſilver in the tube ſoon melted, and conſequently ſubſided. Poſſibly it came much lower than Dr. GMELIN happened to obſerve it at noon; for then the great body of quickſilver was undoubtedly in motion, aſcending rapidly as it expanded by melting, till it came up to the degree that correſponded with the temperature of the air. Therefore, inſtead of a change in that temperature from −46° to −108° in a few hours, and from −108° up to −19° in half an hour, which would have been really aſtoniſhing, this obſervation only ſhews that the cold, after having continued two days as much below −39° as was ſufficient to freeze mercury, at length abated 20 or 30 degrees, perhaps very gradually; no greater alteration than frequently takes place in moſt extra-tropical climates.

A ſimilar inſtance occurred at Kirenga Fort a few days afterwards *, explicable in the ſame manner.

Again, on the 29th of December (O. S.) Dr. GMELIN found his thermometer, which had been ſtanding at −40° early in the morning, ſunk down to −100° at four in the afternoon. He ſubjoins the following remark †. "I obſerved ſome air "in the thermometer, ſeparating the quickſilver for the ſpace "of about ſix degrees. Yeſterday evening I took notice of a "ſimilar appearance, except that the air was not then collected "into one place, but lay ſcattered in ſeveral. I conſidered it "as an accidental fault in the inſtrument, and attempted to "expel it by means of a ſteel wire, but could not bear the "cold. In the barometer, alſo, ſome very ſmall air-bubbles

* Reiſe. Theil. II. p. 625.

†, Ibid. p. 631.

5 "were

"were perceived. Next morning only a very few minute air-"bubbles remained in the quickſilver of the thermometer, "which had then riſen to − 44°, and not the leaſt veſtige of "them was to be ſeen in the barometer."

It cannot be doubted, but theſe appearances proceeded from a congelation of the mercury in Profeſſor GMELIN's inſtruments. His thermometer ſhewed by its deſcent that the cold was ſufficient for this effect; and the diſappearance of thoſe ſuppoſed air-bubbles as the froſt abated, demonſtrates that they were nothing more than interſtices formed by minute portions of congealed metal reſting irregularly upon one another, and which, therefore, were gradually obliterated as the ſolid bits melting down united into one maſs. Now this obſervation is of conſequence, not only as proving that the quickſilver congealed, but likewiſe as pointing out, with great exactneſs, the degree of cold neceſſary for its congelation. For ſince, when only a few very minute bubbles were left, the mercury reached up to − 44° in the thermometer, its freezing point could not be below that degree, becauſe ſome of it continued ſtill ſolid, but muſt be a little higher, juſt ſo much as would anſwer to the expanſion produced by the melting of that very ſmall proportion of metal which remained frozen.

From Dr. GMELIN's attempt to extricate the ſuppoſed air by means of a wire, it would ſeem, that the tubes of his thermometers were open at top. This idea is in ſome meaſure confirmed by a paſſage in the preface to his Flora Sibirica*, where he mentions, that upon arriving at, or quitting a place, he uſed to try whether his thermometer would riſe to 0 in boiling water, and, if there appeared any deficiency, corrected it by the addition of freſh quickſilver; which he would ſcarcely have

* P. lxxiv.

done, had the tube been ſealed, on account of the great riſk of ſpoiling the inſtrument in breaking it open ſo often. Perhaps the above-mentioned appearance of air bubbles, from the divided ſtate of the frozen quickſilver, may have depended in part upon this expoſure to the atmoſphere, as well as upon the large ſize of M. DE L'ISLE's original thermometers. Under theſe circumſtances we cannot ſuppoſe that the inſtruments were very exact.

Dr. GMELIN on ſeveral other occaſions obſerved, that the quickſilver in his thermometer looked as if air was interſperſed in it. Whenever this happened, it always ſubſided many degrees below what we now underſtand to be the point of mercurial congelation *. The profeſſor, totally at a loſs to explain ſuch a phænomenon, imputes it ſometimes to a fundamental fault in his inſtrument, but which he could never diſcover, and at other times to an imaginary effect of the intenſe cold, in expelling or extricating air from the pores of the quickſilver, to be abſorbed as the cold abated. On the 9th of January, 1738, O. S. the mercury ſunk at once to $-114°$, after having been ſtationary two whole days at $-45°$ †.

The laſt obſervations of M. GMELIN's, in which quickſilver froze, were made upon his return homeward in a part of Siberia, much nearer the confines of Europe. During the month of December, 1742, as he was paſſing over that branch of the Ural or Riphæan mountains which runs between Verchoturie and Solikamſk, about the 59th degree of N. lat. and ſcarcely 60 degrees E. of Greenwich, his thermometer ſunk to $-41°$, $-70°$, and at length into the bulb, though it was graduated

* Reiſe. Theil. II. p. 634.

† Ibid.

to

to 96° below o *. The ſame appearance of air bubbles which he had ſo frequently remarked in ſuch great deſcents of the thermometer, puts it beyond doubt that the quickſilver was frozen. This event furniſhed a very ſtriking proof of the force of habit in reconciling men to hardſhips, which in their common courſe of life are thought intolerable. Profeſſor GMELIN, who had now been nine years in Siberia, not only bore to travel in this exceſſive cold, but alſo, in order to aſcertain the height of the mountains he traverſed, employed himſelf in obſerving a barometer, whilſt the quickſilver was freezing in his inſtruments.

Theſe are the principal of Dr. GMELIN's thermometrical obſervations. He collected many more, part of which were deſtroyed by fire or other accidents, and the remainder ſeem to contain no further information. They were conſidered by him as demonſtrating the cold of Siberia to exceed that even of the moſt northern parts of Europe near 100 degrees, an opinion which has ſince been almoſt univerſally adopted; whereas we have, in fact, no proof that the difference of climate amounts to ſo much as the variation between one winter and another. At Yeniſeiſk, where the cold was ſo intenſe in 1735, it does not ſeem to have ever been ſufficient to freeze a thermometer in the winter that M. GMELIN ſpent there four years afterwards; and it will ſoon be ſhewn that quickſilver has congealed more than once in Europe. All that we are authoriſed to conclude, therefore, with reſpect to the Siberian climate, is, that the cold there not unfrequently exceeds the degree indicated by − 39° of a ſtandard mercurial thermometer.

* Reiſe. Theil. IV. p. 512–515.

§ 2.

§ 2. This was the period of ſcientifical enterpriſe. Soon after the intelligent Academicians of Peterſburg had penetrated into Siberia by order of the Ruſſian Monarch, another potent Sovereign ſent out thoſe philoſophical expeditions, by which the opinion of our illuſtrious countryman, reſpecting the figure of the earth, was ſo honourably confirmed. As it became neceſſary, for the determination of this queſtion, to meaſure a degree at the arctic circle, the gentlemen who undertook it were unavoidably expoſed to a great ſeverity of cold. About the time when the quickſilver was exhibited frozen to Profeſſor GMELIN, near the extremity of Aſia, without overcoming his prepoſſeſſion, M. MAUPERTUIS and his aſſociates ſaw the liquor congeal in their ſpirit-thermometer at Torneå in Lapland *. Their mercurial thermometer ſunk at the ſame time to − 37° of M. DE REAUMUR's ſcale; which, if the inſtrument was exactly graduated according to that philoſopher's original idea, would undoubtedly ſhew that the quickſilver froze, as it correſponds with 51° of FAHRENHEIT. But the inaccuracies in conſtructing M. DE REAUMUR's thermometers have been ſo great, that I think no dependence can be placed upon this obſervation, eſpecially as it does not appear to have been attended with any extraordinary phænomenon.

The ſame objection holds good with regard to the obſervations made by M. GAUTIER at Quebec, from the year 1743 to 1749, an extract from which is inſerted in the Memoirs of the French Academy of Sciences †. The account given of his thermometer is too indefinite to allow any certain inference to

* OUTHIER Voyage au Nord, p. 145.

† Mem. de l'Ac. des Scienc. 1744, p. 135; 1745, p. 194.; 1746, p. 88.; 1747, p. 466.; 1750, p. 309.

be

be drawn; but as the quickſilver ſeveral times contracted ſo much as to leave a viſible vacuity in the top of the bulb, and the ſcale ſeems to have reached near to its point of congelation, I am rather of opinion that it actually froze. If ſo, Quebec, ſituated in lat. 47°, is the moſt ſouthern place in which ſuch a great degree of natural cold has hitherto been obſerved.

§ 5. We come now to an inſtance of what, however often it may have happened, has hitherto never been ſuſpected, the congelation of quickſilver in Europe by natural cold. The obſervations which prove this fact are recorded in the Tranſactions of the Royal Academy of Sciences at Stockholm, whence I have extracted the following account, from the original Swediſh.

In January, 1760, the weather was remarkably cold in Lapland. On the fifth of that month different thermometers ſunk to −76°, −128°, or lower *. Again, on the 23d and following days, they fell to −58°, −79°, −92°, and below −238° into the ball †. This great deſcent of the mercury was obſerved in four places, Torneå, Sombio, Iukasierf, and Utsioki, all ſituated between the 65th and 70th degrees of N. lat. and the 21ſt and 28th of eaſtern longitude, by M. ANDREW HELLANT, œconomical Inſpector of Lapland, whoſe remarks on the phænomenon afford of themſelves ſufficient evidence, that the quickſilver was frozen. "During the cold weather at Sombio," ſays he ‡, "as it was clear ſun-ſhine, though ſcarcely "the whole body of the ſun appeared above the low woods that "terminated our horizon, I took a thermometer which was

* Kongl. Vetenſk. Acad. Handlingar. vol. XX. p. 314.

† Ibid. vol. XXI. p. 312.

‡ P. 314.

"hanging

" hanging before in the ſhade, and expoſed it to the riſing ſun " about eleven in the forenoon, to ſee whether, when that lu- " minary was ſo low, it would produce any effect upon the " inſtrument. But to my great ſurpriſe, upon looking at it " about noon, I found that the mercury had entirely ſubſided " into the ball, though it was ſtanding as high as $-61°$ at ele- " ven o'clock, and the ſcale reached down to $238°$ below 0. I " could not perceive or think, that the air had changed ſo ſud- " denly to ſuch an extraordinary degree of cold. I therefore " brought my thermometer, the only one I had left, within " doors, and held it before the fire, when it quickly aſcended " to the uſual height in a warm room. Upon being carried " out again into the open air, and placed in the ſhade, it ſunk, " as in the forenoon, to $-61°$. Afterwards I expoſed it once " more to the ſun-ſhine; but the ſun having already begun to " diſappear behind the horizon, the quickſilver did not ſubſide " into the bulb as before. I then returned with the inſtrument " into the room, and held it in my hands before the fire, upon " which the quickſilver fell back into the ball, where it left a " vacuum or hollow bubble about the ſize of a pepper-corn. " When I inclined the thermometer, this bubble ran round the " ball; and after a few minutes the quickſilver roſe again to " its former height.

" I repeated theſe experiments ſeveral times at Nebuloslock" [a ſettlement about ten miles diſtant from Sombio] " the ſame af- " ternoon and the following day, by carrying the thermometer " out of the cold ſometimes to the fire, and ſometimes into a " warm hut; when the ſame thing happened, that the quick- " ſilver always ſubſided into the ball. After my return to Tor- " neå in April, I attempted to perform them again; but when " the cold was only a few degrees below the freezing point of

" water, I could never make the mercury ſink, either by hold-
" ing it before the fire, or carrying it into a warm room, though
" the experiments always ſucceeded when the thermometer
" previouſly ſtood at − 58° or lower.

" People who were well cloathed, and in briſk motion, or
" driving with rein-deer, could bear this cold an hour or two;
" but ſuch as travelled with horſes in a ſledge ſoon found it ne-
" ceſſary to have recourſe to the farm-houſes to warm them-
" ſelves. On going out from a hot room, ſome of the firſt
" inſpirations were rather heavy and difficult; but the breath-
" ing ſoon became eaſier. It felt dreadfully cold; but ſtill I
" could not perceive, from the ſenſation alone, that the wea-
" ther was ſo extraordinarily ſevere as it appeared to be by the
" inſtruments. Having ſpent an hour at the houſe of one of
" my friends, in the afternoon of the 25th of January, and
" finding on my return that a thermometer, graduated to
" − 58°, had ſunk into the ball, I could not at firſt believe it
" had happened from the cold, but thought the inſtrument
" muſt be broken, till other thermometers that were hanging
" near it convinced me of the truth."

Several reflexions preſent themſelves on the peruſal of theſe obſervations. The phænomena fairly ſhew, that there was a ſufficient degree of cold to congeal the quickſilver in Mr. HELLANT's thermometers, which ſometimes ſunk regularly into the bulb, but commonly ſtuck faſt in the tube till it was heated by the ſun, the fire, or a warm room, and thus made to ſubſide. The continuance of this cold was very remarkable; it laſted no leſs than three days, with ſufficient intenſity to freeze mercury; a circumſtance almoſt unparalleled any where, and the more extraordinary, becauſe M. HELLANT, during twenty-three years that he had made obſervations in Lapland, never

before ſaw the thermometer ſo low as to indicate a congelation of the mercury. But it was not in Lapland alone that the ſeaſon was uncommonly ſevere. At this ſame time the froſt was nearly, if not quite, intenſe enough at Peterſburg to freeze quickſilver, as appears from the remarks of M. BRAUN, who was then engaged in his experiments. And it is a curious coincidence of events, that on the very day when the congelation of mercury by artificial means was firſt clearly eſtabliſhed in Ruſſia, nature ſhould be performing the ſame operation before the eyes of an attentive and philoſophical obſerver in a neighbouring kingdom, who yet had not ſufficient ſagacity to divine her ſecret.

Two circumſtances, however, ſtruck M. HELLANT, which might have led him immediately to ſuſpect the truth. The firſt was, that ſuch a degree of cold as this prodigious deſcent of the thermometer ſeemed to indicate, bore no ſort of proportion to the general ſtile of the weather in that country. What could be more incredible, than that the cold, which had never been known before to ſink the thermometer below $-40°$, ſhould on one particular occaſion exceed that point by hundreds of degrees, more than double the whole variation of temperature between ſummer and winter? Such an event would ſhew a want of balance in the ſyſtem of nature, with reſpect to heat and cold, ſo very different from the apt adjuſtment of its other parts, as to be inadmiſſible but upon the moſt deciſive proofs. Any reflecting perſon, therefore, would be more inclined to believe, that the inſtruments employed had ceaſed to be meaſures of the temperature, from ſome cauſe or other, than that the extremes of cold ſhould be ſubject to ſuch anomalous exceſſes.

The

The other circumſtance of which M. HELLANT takes notice, that the ſenſation attending this cold by no means correſponded with its effect upon a thermometer, pointed directly to the ſame concluſion. When that inſtrument is at 40° below 0, the beams of houſes crack with a loud exploſion, trees ſplit and are killed, birds fall down dead out of the air, and it is with the utmoſt difficulty that man, notwithſtanding all his reſources, can preſerve the extreme parts of his body from being deſtroyed by the froſt. Now, if the cold had been increaſed as far beyond this degree, as *it* differs from the heat of boiling water, could M. HELLANT have expoſed himſelf to the open air with impunity? The analogy of all we know of cold declares the contrary; and though the power of animals and vegetables to reſiſt variations of temperature has been found much greater than was formerly imagined, I think it would not be raſh to affirm, that in any part of our globe where the cold was carried to ſuch exceſs, the whole ſyſtem of organized bodies muſt periſh.

Yet theſe obvious inferences ſeem to have never occurred to M. HELLANT. Even the unexpected deſcent of his thermometer on being expoſed to heat, ſtrange and inexplicable as it muſt have appeared, and contradictory to all the notions he entertained, did not ſuggeſt to him a doubt of the inſtrument's marking the real temperature of the air. But we now know that it ceaſed to do ſo after the cold had increaſed a few degrees below $-39°$; that all the unuſual phænomena turned upon the congelation of the quickſilver; and that the ſeverity of this ſeaſon, though greater than uſual in Lapland, did not exceed that of common winters by any ſuch remarkable difference.

The vacuum or hollow bubble, obſerved after the quickſilver had fallen back into the ball, ſhews how very much it had contracted by the congelation. This bubble moved upon inclining the

thermometer, becauſe the external part of the frozen maſs in the ball having melted before the internal, though not in ſufficient quantity to fill its whole capacity, ran round it freely in a fluid ſtate, the empty ſpot always riſing to the top.

§ 4. Early in the ſpring of 1761, the Abbé CHAPPE D'AUTEROCHE, in his journey to Tobolſk for obſerving the tranſit of Venus, paſſed through Solikamſk, a town of Siberia, ſituated in 59°½ N. lat. and 57° E. of Greenwich. On this occaſion he takes notice *, that the thermometer had ſunk there the preceding winter to −124°; which, if the general ſtile of the Abbé's remarks will allow ſufficient dependence to be placed upon it, would neceſſarily ſhew that the quickſilver was then frozen.

§ 5. M. ERICH LAXMANN, late Profeſſor of Mineralogy and Chemiſtry at Peterſburg, was reſident in 1765 at Barnaul in Siberia, lat. 53° N. and long. 81° E. as miniſter to the German congregation of the Kolyvan Province. On the firſt day of that year, he ſaw the thermometer down ſo low as −58° †; whence it is probable, that ſome part at leaſt of the quickſilver was congealed. As no concomitant circumſtances are recorded with this fact, it would ſcarcely have been worth mentioning, were it not to introduce an account of ſome inſtruments, which became afterwards the ſubject of very curious obſervations. For M. LAXMANN, during his abode in this remote country, employed his leiſure in the conſtruction of barometers and thermometers, an art in which he acquired great ſkill. Theſe he afterwards diſtributed, free of expence, to all

* Voyage en Siberie, p. 84. and 93.

† LAXMANN's Sibiriſche Briefe, p. 97.

parts

parts of Siberia where they were likely to be uſed, with the moſt laudable and exemplary zeal to diffuſe ſome rays of ſcience through thoſe dark and uncultivated regions *.

All the particulars here mentioned are extracted from M. LAXMANN's *Siberian Letters*; a ſcarce book, becauſe, on his return to Europe, he bought up every copy he could find, as they had been publiſhed without his conſent by Profeſſor SCHLÖZER of Gottingen.

§ 6. The benefits accruing from the travels of learned men, could not eſcape the penetration of the wiſe Empreſs who now reigns in Peterſburg. Soon after her eſtabliſhment on the throne, ſhe ordered an expedition of the ſame nature as that in which Profeſſor GMELIN had been engaged above thirty years before. Among the gentlemen who undertook this ſecond philoſophical ſurvey of the Ruſſian Empire, was Dr. PETER SIMON PALLAS, one of the moſt eminent naturaliſts and ſkilful obſervers of the preſent age. The journal of his travels is publiſhed by himſelf in the German language, and comprehends a rich ſtore of curious and uſeful information. In general his winters were not ſpent in the coldeſt parts of Aſia; twice, however, he reſided at Kraſnoyarſk lat. 56°½ N. long. 93° E. and the laſt time, in 1772, had an opportunity of witneſſing the moſt remarkable inſtance of the congelation of mercury by natural cold that is yet known to the world.

"The winter," ſays M. PALLAS †, "ſet in early this year, "and was felt in December with uncommon ſeverity. On the "6th and 7th of that month happened the greateſt cold I have "ever experienced in Siberia; the air was calm at the time,

* Sibiriſche Briefe, p. 29.

† Reiſe durch verſchiedene Provinzen des Ruſſiſchen Reichs, Theil. III. p. 417.

" and ſeemingly thickened, ſo that, though the ſky was in
" other reſpects clear, the ſun appeared as through a fog. I
" had only one ſmall thermometer left, on which the ſcale
" went no lower than $-70°$; and on the 6th in the morning I
" remarked that the quickſilver in it ſunk into the ball, except
" ſome ſmall columns which became ſolid and ſtuck faſt in the
" tube. By the temperature of a room not much warmed,
" into which I brought the thermometer from the gallery of
" my houſe, theſe congealed columns immediately fell down;
" but it was more than half a minute before the mercury came
" into motion out of the ball. I repeated this experiment fre-
" quently, and always with ſimilar ſucceſs, ſometimes one and
" ſometimes more threads of frozen quickſilver remaining behind
" in the tube. When the ball of the thermometer, as it hung
" in the open air, was warmed by being touched with the fin-
" gers, the quickſilver roſe; and it could plainly be ſeen, that
" the ſolid frozen columns ſtuck and reſiſted a good while, and
" were at length puſhed up with a ſort of violence. In the
" mean time I placed upon the gallery on the north ſide of my
" houſe about a quarter of a pound of clean and dry quick-
" ſilver in an open bowl; within an hour I found the edges
" and ſurface of it frozen ſolid, and ſome minutes afterwards
" the whole was condenſed, by the natural cold, into a ſoft
" maſs very much like tin. While the inner part was ſtill
" fluid, the frozen ſurface exhibited a great variety of *branched*
" wrinkles; but in general it remained pretty ſmooth in freez-
" ing, as did alſo a larger quantity of quickſilver which I after-
" wards expoſed to the cold. The congealed mercury was more
" flexible than lead; but upon being bent ſhort it was found
" more brittle than tin, and when hammered out thin it ſeemed
" ſomewhat granulated. If the hammer had not been per-

" fectly cooled, the quickſilver melted away under it in drops;
" and the ſame thing happened when the metal was touched
" with the finger, by which alſo the finger was immediately
" benumbed. In our warm room it thawed on its ſurface gra-
" dually, by drops, like wax on the fire, and did not melt all
" at once. When the frozen maſs was broken to pieces in the
" cold, the fragments adhered to one another, and to the bowl
" in which they lay. Although the froſt ſeemed to abate a
" little toward night, yet the congealed quickſilver remained
" unaltered, and the experiment with the thermometer could
" ſtill be repeated. On the 7th of December I had an oppor-
" tunity of making the ſame obſervations all day; but ſome
" hours after ſun-ſet a north-weſt wind ſprung up, which raiſed
" the thermometer to $-46°$, when the maſs of quickſilver
" began to melt."

Before this obſervation of Dr. PALLAS's, no perſon had ſeen or handled quickſilver frozen by natural cold, ſo as to ſubmit the fact to the public with competent evidence; but the circumſtances here related are ſo pointed and conſiſtent, that even thoſe who had doubted of M. BRAUN's experiments were now ſtaggered, and began to believe. Indeed, it was ſcarcely poſſible to ſuppoſe any miſtake, when Dr. PALLAS had two whole days to repeat and vary the experiments at his leiſure. But beſides removing all doubts upon the congelation of quickſilver, theſe obſervations tended to ſhew, within certain limits, the degree of cold neceſſary for that effect. It was evident that the freezing point muſt be ſomewhere above 70°, becauſe the thermometer's graduation reached only ſo low, and yet ſome part of the mercury always congealed in the tube; and as the ſolid maſſes did not begin to melt till the thermometer roſe to $-46°$, *that* ſeemed to be nearly the point at which it paſſes

from

from a ſolid to a fluid ſtate, and very poſſibly was ſo upon this inſtrument, a difference of ſeveral degrees being often found in thermometers ſo low down on the ſcale as -40°, from inaccuracies in their conſtruction.

The cryſtallization of quickſilver, alſo, became manifeſt on this occaſion. Hence, when hammered out thin, it ſhewed a granulated texture. The branched wrinkles too, which formed on its ſurface whilſt it was congealing, could ſcarcely have proceeded from any other cauſe, and ſuggeſt a general idea of the manner in which it ſhoots. That quickſilver ſhould cryſtallize ſo much more viſibly than moſt other metals, will not appear ſurpriſing, if we conſider how little the cold is below its freezing point. Such ſubſtances as require, in order to melt, a degree of heat much above that of our atmoſphere, experience ſo great a change of temperature upon being taken off the fire, that they become ſolid haſtily, and as it were in confuſion; whereas quickſilver, having never probably been expoſed to a degree of cold much exceeding that of its melting point, its particles have had full leiſure to arrange themſelves regularly, in exact conformity to the laws of their mutual attractions. As in Profeſſor BLUMENBACH's and Mr. HUTCHINS's experiments, ſo here, I imagine, ſome ſlight roughneſs of the ſurface was occaſioned by this cryſtallization; in conſequence of which M. PALLAS compared his frozen quickſilver to tin, rather than to bright ſilver, the appearance it always aſſumes when congealed in ſmooth glaſs.

Another property of quickſilver, very important to be known, was obſerved perhaps no where ſo diſtinctly as on this occaſion at Kraſnoyarſk; I mean its tendency to adheſion in freezing. Thus, Dr. PALLAS ſays the fragments of the congealed maſs ſtuck to one another, and to the bowl in which they

they lay. So likewiſe Mr. HUTCHINS found the frozen quickſilver adhering to his cylinders and gallipot; Profeſſor BLUMENBACH to his glaſs veſſel; and ſimilar facts occurred to other obſervers. Hence the deceptions, already ſo often mentioned, from the ſticking of the mercury in the ſtems of thermometers. And this cauſe of error can ſcarcely ever fail to take place; for if quickſilver congealing in wide open veſſels adheres to them wherever it touches, how can it be expected to remain looſe when frozen in a narrow tube? Now, ſince quickſilver, under theſe circumſtances, retains the ſame appearance as while fluid, from the poliſh given to its ſurface by the ſmooth glaſs, it is no wonder that ſuch frequent miſtakes have been made relative to the height of the thermometer, both in experiments with artificial cold, and in meteorological obſervations. At the ſame time it muſt be confeſſed, that ſuch a tendency to adhere, in a metal which contracts ſo much in becoming ſolid, is not a little difficult to explain, unleſs we may ſuppoſe it to be the immediate effect of the cryſtallization.

Quickſilver, with all its other qualities of a perfect metal, ſeems from Dr. PALLAS's, and indeed moſt of the experiments, not to be completely malleable, but rather apt to break under the hammer. Perhaps it has never been ſufficiently cooled to poſſeſs its metallic properties in perfection; for with reſpect to its melting point it may be conſidered as having always been hot, that is, heated near to fuſion, a ſtate in which other metals undergo a very ſenſible change in their properties. But when mercury congeals in veſſels which confine its ſurface, it ſeems to become more malleable than under a looſe cryſtallization.

Dr. PALLAS's travels laſted from the year 1768 to 1773, during which time *this* of the beginning of December, 1772,

ſeems to be the only inſtance that occurred of the freezing of quickſilver. From this circumſtance, and his occaſional hints about the climate, I am inclined to ſuſpect, that the cold was not ſo great in Siberia at this period, as when Dr. GMELIN was there from 1734 to 1742. In Europe, likewiſe, the winters in general appear to have been ſeverer about the time of GMELIN's expedition than lately. It was in this very town of Kraſnoyarſk that Profeſſor GMELIN reſided during the famous hard winter of 1739 and 1740; but unfortunately he has informed us of the cold there only by its groſſer effects, I believe becauſe his thermometers had been accidentally deſtroyed.

§ 7. Nearly 500 miles ſouth-eaſtward of Kraſnoyarſk is the town of Irkutſk, the capital of a Siberian Province on the vaſt Baikal Lake, and ſituated in lat. 52° N. and about the 104th degree of E. longitude. At the former of theſe places, the cold recorded by Dr. PALLAS began to abate on the 7th of December in the evening; and more than a day after, that is, on the 9th in the morning, it became ſo intenſe at Irkutſk as to freeze quickſilver. An account of this phænomenon, ſent by Lieutenant-general VON BRILL, the governor, was publiſhed by Dr. PALLAS in his Journal*, and M. GEORGI, one of the aſſociates in this expedition, afterwards collected ſome further particulars†. It appears, that about four in the morning, the quickſilver was found frozen in the barometer and thermometer, its upper ſurface being irregularly broken. In the former of theſe inſtruments the mercury ſtood at 28 inches 7 lines, and the broken appearance extended through a ſpace of about

* Ibid. p. 419.

† Bemerkungen einer Reiſe im Ruſſiſchen Reich, B. I. p. 29.

5 lines

5 lines from the top downward: when it came to melt a few hours afterwards, it roſe to 29 inches 7 lines, which difference of height was probably in part at leaſt an effect of the contraction it undergoes in freezing, its greater ſpecific gravity in the congealed ſtate making it ſtand proportionably lower. In the thermometer, part of the mercury had ſtuck at $-44°$; and immediately under $-59°$ an empty ſpace was left, equal to 11 degrees of the ſcale. This obſervation, therefore, determines almoſt preciſely the freezing point; for none of the quickſilver could have adhered in the tube ſo high as $-44°$, unleſs it had congealed before it ſunk below this point, and conſequently before the cold exceeded this degree. And that the mercury was really frozen became evident afterwards; for about eleven in the forenoon, as the air grew warmer, it was found to have all ſubſided into the bulb, the ſmall threads in the tube melting down into the vacuity left there, out of which it did not riſe again till near two hours had elapſed.

The accuracy of this thermometer at Irkutſk, apparent from its preciſion with reſpect to the point of congelation, would be matter of ſurpriſe, had we not been informed, that both it and the barometer were among the inſtruments conſtructed by M. LAXMANN during his reſidence in Siberia. M. GEORGI mentions them as being in the poſſeſſion of Dr. WACHSMANN, the public phyſician of Irkutſk, by whom probably the obſervations were communicated to the governor.

§ 8. As the cold of America is well known to exceed that which prevails under the ſame latitudes in Europe, we muſt expect to find quickſilver freezing ſpontaneouſly in parts of that continent which do not lie very far to the northward. Accordingly, beſides the inſtance of Quebec formerly mentioned,

this effect takes place frequently in Hudſon's Bay, even at Albany Fort, where the latitude is not one degree greater than in London. Mr. HUTCHINS, in his different ſituations at Hudſon's Bay, has been conſtantly attentive to meteorological obſervations. During his former reſidence at York Fort, ſituated near the middle of the Bay in lat. 58 N. he was not provided with any thermometer graduated more than 70 or 90 degrees below the cypher: but he remarked, that "the quickſilver "frequently ſunk into the bulb," eſpecially after having been ſtationary at $-55°$ or $-57°$, and that it afterwards uſed to aſcend to "about $-30°$, indicating a greater degree of "heat than before it fell *." Theſe phænomena were clearly owing to its congelation, adheſion in the tube, and ſubſequent liquefaction as the air grew warmer. When Mr. HUTCHINS went afterwards to Albany Fort, and had procured inſtruments with more extenſive ſcales, he obſerved the ſame appearances ſtill more diſtinctly. His thermometers froze twice in the winter of 1774 and 1775, and three times in that of 1777 and 1778; and in every inſtance, except one, the mercury ſunk hundreds of degrees juſt as the cold began to abate †. The laſt of theſe obſervations is rendered remarkable by the deſcent of the quickſilver to $-490°$, the greateſt ever known by natural cold, and probably very near its extreme term of contraction by freezing. In 1782, alſo, Mr. HUTCHINS's thermometers, together with ſome quickſilver in a phial, again congealed in the open air, and exhibited ſimilar phænomena, as appears from the account of his experiments.

Fortunately in theſe inſtances of intenſe cold at Albany Fort, attention was paid not only to the mercurial thermometers, but

* MS. Journal.

† Ibid.

likewiſe

likewiſe to one made of ſpirits, whoſe relative movement has been aſcertained by compariſon. This inſtrument, while the others were three, four, or almoſt five hundred degrees below 0, never ſunk further than to a point which correſponds with $-46°$ of a ſtandard mercurial thermometer. Hence it would have been eaſy to infer, both that the quickſilver actually congealed on theſe occaſions, and that the degree of cold neceſſary for ſuch an effect does not exceed $-46°$. That the moſt intenſe cold of Hudſon's Bay, during a ſeries of ſeveral years, went ſo little below the point of mercurial congelation, well deſerves to be noticed; and as it ſeems to be ſeldom greater in Siberia, at leaſt in the parts viſited by GMELIN or PALLAS, the effects being not more violent, perhaps we are authoriſed to conclude, that the extreme of artificial cold, produced by ſnow and nitrous acid, correſponds pretty exactly with the extreme of natural cold in the moſt rigorous climates which can well be inhabited.

Mr. HUTCHINS's meteorological journal at Hudſon's Bay confirms what he mentions in the remarks on his third experiment, that in the coldeſt weather the wind is to the ſouthward of W. which muſt evidently depend on ſome local circumſtance of that country. During my own reſidence in America, I thought I could diſtinctly perceive that the coldeſt winds, vulgarly called *north-weſters*, did not blow exactly from that quarter; but at Rhode Iſland, for inſtance, came from N.W. by W. or a point ſtill more to the weſtward; at New York were rather nearer the W. than the N.; and at Philadelphia almoſt due N.W. On tracing lines in theſe directions from Albany Fort, Rhode Iſland, and Philadelphia, they are found to meet among the great ridges of mountains which ſeparate Hudſon's Bay from Lake Superior; whence a ſuſpicion ariſes, that theſe mountains

mountains may be one ſource of the exceſſive cold which particular winds occaſion in North America. But another circumſtance is to be taken into conſideration, that when the fury of theſe N.W. winds abates, they very regularly draw round to the weſtward and ſouthward. Now the *maximum* of cold muſt be, when the cold wind has blown as long as it can, and the ſucceeding warmer wind has not yet had time to undo any of its effect; conſequently at the period when it is veering round from the northward toward the ſun. Accordingly we find, that when the cold has become ſufficiently great at Albany Fort to congeal quickſilver, the wind not only came from the ſouthward of W. but commonly, alſo, blew with very little force; indeed the intenſeſt froſts in all countries ſeem to take place with light airs or a calm.

§ 9. There are, in the poſſeſſion of the Royal Society, ſeveral other meteorological regiſters from our different ſettlements on Hudſon's Bay; but none of thoſe which I have ſeen contain any ſtriking extraordinary appearances, to ſhew that the thermometer was frozen; and the deſcent alone, within certain bounds, determines nothing, for theſe inſtruments were formerly conſtructed with ſo little accuracy, as to be often marked 8 or 10 degrees too low at the point of mercurial congelation. I conclude, therefore, that when Meſſ. WALES and DYMOND were at Prince of Wales's Fort in the winter of 1768 and 1769, the quickſilver always retained its fluidity, though it once ſunk to −45° of their thermometer*. This Fort lies in lat. 59°; but on account of the abovementioned ridges of mountains, and perhaps for other cauſes, the ſouthern parts of the Bay may be quite as cold as the northern.

* Phil. Tranſact. vol. LX. p. 153.

§ 10.

§ 10. We muſt now return to Europe. In the beginning of the year 1780, M. VON ELTERLEIN, of Vytegra, froze quickſilver by natural cold, and ſent an account of his experiment in a letter to the late Profeſſor GÜLDENSTÄDT, then at Peterſburg. I obtained from Gottingen a copy of this letter, in the original German, by the friendſhip of Sir JOSEPH BANKS; and, tranſlated, it is as follows:

"On the 4th of January, 1780, the cold having increaſed "to −34° that evening at Vytegra, I expoſed to the open air "three ounces of very pure quickſilver, in a China tea-cup, "covered with paper pierced full of holes. Next day, at eight "in the morning, I found it ſolid, and looking like a piece of "caſt lead, with a conſiderable depreſſion in the middle. On "attempting to looſen it in the cup, my knife raiſed ſhavings "from it as if it had been lead, which remained ſticking "up; and at length the whole ſeparated from the bottom "of the cup in one maſs. I then took it in my hand to "try if it would bend; it was like ſtiff glue, and broke "into two pieces; but my fingers immediately loſt all feeling, "and could ſcarcely be reſtored in an hour and an half by rub-"bing with ſnow. At eight o'clock a thermometer, made by "M. LAXMANN of the Academy, ſtood at −57°; by half after "nine it was riſen to −40°; and then the two pieces of mer-"cury, which lay in the cup, had loſt ſo much of their hard-"neſs that they could no longer be broken or cut into ſhavings, "but reſembled a thick amalgam, which, though it became "fluid when preſſed by the fingers, immediately afterwards "reſumed the conſiſtence of pap. With the thermometer at "−39°, the quickſilver became fluid. The cold was never leſs "on the 5th than −28°, and by nine in the evening it had

"increaſed

"increaſed again to −33°. In the morning the wind was "N.N.E. and afterwards N.W."

This experiment of M. VON ELTERLEIN's deſerves attention in many reſpects. It aſcertains the freezing point of mercury with ſuch wonderful exactneſs, from the melting of the ſolid pieces when the thermometer came up to −39°, as to furniſh a valuable corroboration of Mr. HUTCHINS's experiments, and at the ſame time very much to enhance our opinion of M. LAXMANN's ſkill in the conſtruction of inſtruments. When the thermometer was thought, early in the morning, to be ſtanding at −57°, I imagine, that part of the quickſilver being frozen adhered in the tube. The ductility of the ſolid metal muſt have been conſiderable, from its yielding to the knife in the form of ſhavings; yet, as in moſt other inſtances, it ſhewed ſome degree of brittleneſs when force was applied to it in the maſs. Cryſtallizing without impediment, it aſſumed an appearance which M. VON ELTERLEIN rather compares to that of lead than of ſilver. Its tendency to adheſion became evident from the neceſſity of employing an inſtrument to ſeparate it from the tea-cup; and its contraction in freezing was demonſtrated by the depreſſion obſerved in the middle of the ſolid maſs. This ſingle experiment, therefore, exemplifies, in a very beautiful manner, moſt of the properties hitherto diſcovered in quickſilver, when it paſſes from a fluid to a ſolid form.

Vytegra, or Witegorſk, is ſituated in lat. 61° N. and long. 36° E. upon a river of the ſame name. It has acquired ſome celebrity from one of the many uſeful projects which occupied the active mind of Czar PETER the Great. He propoſed to cut a canal from the river Vytegra which diſcharges itſelf into the Lake Onega, to the river Kovſha which joins the Beloſero, or White Lake, in order to form a communication between thoſe

two

two great bodies of water; but the undertaking was unfortunately interrupted by his death *.

§ 11. The laſt inſtance I have been able to find of the congelation of quickſilver by natural cold, occurred no longer ago than the beginning of the year 1782, in Iemtland, one of the northern provinces of Sweden. M. JOHN TÖRNSTEN, Engineer-extraordinary, is the gentleman to whom we are indebted for this obſervation. His letter on the ſubject, dated from Brunflo in Iemtland, lat. 63° ¼ N. and long. 15° E. is inſerted in the Swediſh Tranſactions for 1782 †, together with ſome remarks upon it by Profeſſor WILCKE.

"During twelve years," ſays M. TÖRNSTEN, "that I have "reſided here in Iemtland, the cold had never but once brought "the thermometer ſo low as − 36°, till the laſt day of Decem- "ber, 1781, when it fell in the evening to − 54°. The fol- "lowing new-year's day it was ſunk to − 56° at eight in the "morning, and by ten to − 62°. Here it continued ſtationary "ſeveral hours, but at half paſt four in the afternoon it was "obſerved at − 116°, and by eight the ſame evening it had "riſen to − 31°. Although the quickſilver," continues M. TÖRNSTEN, "thus fell to − 116° on the firſt of January in "the afternoon, I am of opinion that its deſcent ought not to "be aſcribed to a proportionable increaſe of cold, but on the "contrary proceeded from the ſudden change to milder wea- "ther, which came on that afternoon. For the preceding "evening, when the thermometer was ſtanding at − 54°, I "remarked, that, upon bringing it into a warm room, the "quickſilver fell on a ſudden entirely into the ball, which was

* Büſching's Erdbeſchreibung, Theil I. p. 669.

† Kongl. Vetenſk. Acad. Nya Handlingar, tom. III. p. 80.

" about 130 degrees below 0. This experiment I repeated ſe-
" veral times with ſucceſs, but obſerved the following differ-
" ence, that if I had not kept the thermometer in the heat long
" enough for the quickſilver to begin to riſe again after it had
" ſunk into the ball, it never aſcended above the 130th degree
" by continuing in the cold, but upon being carried back into
" the warm room it contracted ſtill more in the ball by a quan-
" tity which, however viſible, could not be meaſured. On
" the other hand, if the inſtrument had been kept in the room
" till the mercury had riſen above − 54°, it became ſtationary
" at that degree in the open air. Now, though I did not, on
" the 1ſt of January, bring the thermometer within doors be-
" fore it had ſunk of itſelf to − 116°, yet *this* fall likewiſe
" ſeems to have been occaſioned by the change to milder wea-
" ther which was then taking place. For at eight in the even-
" ing, when the external cold was at − 31°, I found that hoar-
" froſt formed on the ball and ſtem of the thermometer as be-
" fore, upon its being brought into a warm room; but the
" mercury did not ſink, on the contrary it began immediately
" to riſe.

" Some quadrupeds periſhed by the intenſe cold, and a great
" number of ſmall birds were found dead. Nevertheleſs, the
" people did not neglect going to church on this high holiday,
" and I have not heard that any one was froſt-bitten who went
" out with proper cloathing."

M. TÖRNSTEN certainly judged right when he concluded, that the fall of the thermometer to − 116° rather indicated a diminution than an increaſe of the cold. Though he knew nothing of the cauſe, yet his obſervation led him to a juſt inference, in which he diſplayed more ſagacity than M. HELLANT on a ſimilar occaſion. All the phænomena which ſo much perplexed

theſe

theſe gentlemen are explicable in the following manner. When the air becomes ſufficiently cold to freeze quickſilver, that metal muſt be ſtanding about − 39°, or, in the common way of marking the boiling point, ſomewhere between − 40° and − 50°, in the tube of a thermometer expoſed to it. As the ſmall thread of mercury in the tube muſt be more eaſily affected by the cold, it will probably congeal before any other part, and ſtick faſt about the abovementioned degrees. The remainder of the mercury will then go on to freeze, and as it ſuffers ſuch a great contraction in becoming ſolid, muſt leave a conſiderable vacuity in the bulb of any common thermometer. Conſequently, when the cold, from whatever cauſe, comes to be leſs than is required for keeping the metal in a ſolid ſtate, the ſmall thread that was frozen in the tube immediately melts, and ſinks down into the vacuity of the bulb, where the whole maſs remains, till by its gradual liquefaction it expands again into the tube, and becomes a juſt meaſure of the temperature. This agrees exactly with what M. TÖRNSTEN obſerved. In the evening of the 31ſt the quickſilver congealed in his thermometer, and part of it ſtuck in the tube at − 54°, but ſubſided into the vacuity left in the bulb, as ſoon as it was expoſed to heat. When the inſtrument had been kept in the warm room till the quickſilver re-aſcended into the tube, it froze and adhered again in the open air, and the ſame phænomena were repeated. If M. TÖRNSTEN be exact in ſaying it always became faſt at − 54°, the circumſtance is curious, and may have depended upon ſome particular ſtate of the tube in that part, or upon the firſt ſhooting of the mercury after it had been cooled to a certain degree below its freezing-point. But when the thermometer was carried back into the open air before any of the quickſilver had riſen out of the bulb, the effect of the cold could not be to

force it up into the tube, and therefore no ſuch appearances were obſerved as in the former caſe. With regard to M. TÖRNSTEN's remark, that when the whole maſs of quickſilver remained in the ball it ſtill contracted upon the application of heat, the fact is ſo improbable, and would be perceived with ſuch difficulty, that I have no doubt but he was miſled by ſome prepoſſeſſion. In like manner on the 1ſt of January, when the thermometer, having been ſtationary ſome hours at $-62°$, ſunk in the afternoon to $-116°$, it happened unqueſtionably from the melting and ſubſiding of a thread of frozen mercury, which had adhered in the tube of the inſtrument as high as the former degree. None of theſe effects could be produced when the thermometer had riſen to $-31°$, becauſe the cold was not then ſufficient to congeal the quickſilver. In this eaſy and ſimple manner, does our knowledge of the freezing point of mercury enable us to account for phænomena, which were thought ſo anomalous as to elude every kind of explanation. Even ſo lately as laſt year, one of the moſt eminent philoſophers in Europe, Profeſſor WILCKE of Stockholm, made a vain attempt to ſolve the difficulties by a ſtrained application of his doctrine relative to the various ſpecific quantities of heat in bodies, and their different attractions for the matter of heat*.

It would now be ſuperfluous to add, that the real cold at Brunſlo was by no means what the thermometer ſeemed to indicate, but probably very little exceeded $-39°$, or the degree of mercurial congelation, had not M. TÖRNSTEN's obſervations been lately repreſented, even in this country, as exhibiting an inſtance of cold actually carried to ſuch a diſproportionate and enormous exceſs.

* Ibid.

Thus

Thus is the hiſtory of the congelation of quickſilver, both by natural and artificial cold, brought down to the preſent period. All the facts I have collected are here delivered: it is not improbable, however, that there may be others which have eſcaped me, eſpecially ſuch as are very recent, or have never been publiſhed *; but the number already found is greater than

* Accordingly, having gone to Paris after this paper was read, and there mentioned, at a meeting of the Academy of Sciences, our late experiments on the congelation of quickſilver, I was informed, that M. CAZALET had ſucceeded in rendering it ſolid at Bourdeaux; and ſoon afterwards the following account of his experiment came out in the Paris Journal, which, for obvious reaſons, I ſhall give in the original French.

" M. CAVENDISH, de la Société Royale de Londres, a fait dans le mois de " Février dernier, l'experience de la congélation du mercure, à Hampſtead, ſitué à " deux milles de Londres. Ce Savant eſt ſur le point de publier ſon mémoire à " ce ſujet. Le travail du chymiſte Anglois ayant été annoncé à l'Academie " Royale des Sciences, dans une de ſes ſéances, ſon Directeur, M. CADET DE " GASSICOURT, a *revendiqué* en faveur de M. CAZALET, la congélation du mer- " cure dans un climat beaucoup plus temperé que Londres, le chymiſte François " l'ayant obtenue à Bordeaux. . . . . .

" M. CAZALET, regardant l'acide nitreux concentré comme, de tous les ſels, " celui qui produit la diſſolution de la glace avec plus de facilité, annonça en 1779, " dans une de ſes leçons publiques à Bordeaux, qu'il croyoit à la poſſibilité de la " congélation du mercure, par ce moyen, dans les Provinces méridionales mêmes; " mais il n'eut occaſion de faire ſon expérience qu'au mois de Septembre de " l'année dernière.

" Il prit de la glace pilée, paſſée à travers un crible, la mit dans un baril dont " le fond étoit un plat de porcelaine percé de pluſieurs trous pour faciliter l'ecoule- " ment de la glace fondue; il plaça pluſieurs tubes remplis de mercure au centre " de la glace, qu'on arroſa d'eſprit de nitre fumant fait par le procédé de WOULFE. " On rapprochoit la glace des tubes à meſure que la diſſolution s'en opéroit; il " fallut 120 livres de glace pour produire la congélation du mercure. Les tubes " retirés et caſſés les uns après les autres, le mercure ſe trouva en filets comme

" de

than I expected on beginning the ſearch. By ſuch a connected view of the different obſervations and experiments in any one branch of ſcience, we are furniſhed with the beſt opportunity of diſcriminating what is certain from what is doubtful, and acquire as diſtinct ideas as the actual ſtate of knowledge will admit. On the preſent ſubject of mercurial congelation, the concluſions have in general been noticed, as the premiſes occurred. Though Mr. HUTCHINS's experiments did not ſtand in need of any confirmation, yet ſtill it is pleaſant to ſee their principal reſult, the freezing point of quickſilver, eſtabliſhed by ſuch a body of collateral evidence as, taken together, is abſolutely irreſiſtible. But beſides the information obtained relative to quickſilver itſelf, we have been able to correct ſeveral vulgar prejudices. The difference between cold climates no longer appears ſo prodigious, nor the reſiſting powers of animals and vegetables ſo aſtoniſhing and inconceivable. That extenſive ſcale of heat, which repreſents its diminutions by artificial means as continued down ſo many hundreds of degrees below the greateſt produced by nature, however ſpecious in proſpect, proves to be deſtitute of foundation. The uſe of quickſilver for thermometers is at length fully aſcertained. From the boiling point, to 39° or 40° below 0, it muſt be conſidered as unexceptionable, all ſuſpicion of its irregular contraction within thoſe

" de l'argent. Bientôt la chaleur de l'atmoſphére lui rendit ſa fluidité. *Cette* " *experience intereſſante a le double merite d'avoir été devinée par un chymiſte Fran-* " *çois, et exécutée par un autre; il a fuppléé à ce que le climat oppoſoit d'obſtacles,* " *un moyen tout-à-fait ingenieux, et qui exigeoit des connoiſſances en phyſique.*" See Journal de Paris, 15 Juill. 1783, p. 814.

The peculiarity of M. CAZALET's proceſs conſiſts in the largeneſs of the quantities on which he operated, and the proviſion he made for the uſeleſs liquor, produced by the melted ice, to run off as faſt as it was formed. He certainly congealed the mercury, but did nothing to aſcertain its freezing point, which he ſeems not even to have had in contemplation.

bounds being removed, by ſuch a complete explanation of the cauſe upon which its anomalous deſcent in the lower part of the ſcale depends. On this principle there might, perhaps, be ſome propriety, in conſtructing thermometers of mercury, to fix the cypher at its point of congelation, and thence reckon the degrees of heat upwards.

The principal advantage, however, of thus paſſing in review all former accounts, is to furniſh an important leſſon to authors, which can never be too ſtrongly inculcated, that their accuracy muſt be brought to the teſt of future diſcoveries. As knowledge advances, their errors, their miſrepreſentations, their ſuppreſſion of the truth, or fictitious additions to it, ſhall all be infallibly detected, and heap upon their head proportionable ignominy; while the ſimple and candid narrative, the exact and unbiaſſed relation of facts, will acquire redoubled luſtre from the fiery trial. Let every author recollect, that the day is impending, when ſome unforeſeen improvement, affording means to ſift falſehood from truth, however artfully blended, ſhall finally decide whether he is to be reprobated with the baſe herd of deceivers, or ranked among thoſe faithful votaries of ſcience, whoſe names will be delivered down with honour to poſterity.

XXII. *Experiments relating to Phlogiſton, and the ſeeming Converſion of Water into Air.* By Joſeph Prieſtley, *LL. D. F. R. S.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 26, 1783.

TO SIR JOSEPH BANKS, BART, P. R. S.

DEAR SIR,

AT the perſuaſion of my friends, I beg you would lay before the Royal Society my late obſervations on *Phlogiſton*, and alſo on the ſeeming *Converſion of water into air*, though I have by no means done all that I have in view with reſpect to theſe ſubjects. The principal *facts* are, I think, ſufficiently aſcertained, though I do not preſume to give any opinion with reſpect to the *theory* of them.

I am, with the greateſt reſpect, &c.

Birmingham, April 21, 1783.

Expe-

Experiments relating to Phlogiſton.

THERE are few ſubjects, perhaps none, that have occaſioned more perplexity to chemiſts than that of *phlogiſton*, or, as it is ſometimes called, *the principle of inflammability*. It was the great diſcovery of STAHL, that this principle, whatever it be, is transferrable from one ſubſtance to another, how different ſoever in their other properties, ſuch as ſulphur, wood, and all the metals, and therefore is the ſame thing in them all. But what has given an air of myſtery to this ſubject, has been that it was imagined, that this principle, or ſubſtance, could not be exhibited except in combination with other ſubſtances, and could not be made to aſſume ſeparately either a fluid or ſolid form. It was alſo aſſerted by ſome, that phlogiſton was ſo far from adding to the weight of bodies, that the addition of it made them really lighter than they were before; on which account they choſe to call it *the principle of levity*. This opinion had great patrons.

Of late it has been the opinion of many celebrated chemiſts, Mr. LAVOISIER among others, that the whole doctrine of phlogiſton had been founded on miſtake, and that in all caſes in which it was thought that bodies parted with the principle of phlogiſton, they in fact loſt nothing, but on the contrary, acquired ſomething; and in moſt caſes an addition of ſome kind of air; that a metal, for inſtance, was not a combination of two things, *viz.* an earth and phlogiſton, but was probably a ſimple ſubſtance in its metallic ſtate; and that the calx is produced not by the loſs of phlogiſton, or of any thing elſe, but by the acquiſition of air.

The arguments in favour of this opinion, eſpecially thoſe which are drawn from the experiments of Mr. LAVOISIER made on mercury, are ſo ſpecious, that I own I was myſelf much inclined to adopt it. My friend Mr. KIRWAN, indeed, always held that phlogiſton was the ſame thing with inflammable air; and he has ſufficiently proved this from many experiments and obſervations, my own as well as thoſe of others. I did not, however, accede to it till I diſcovered it by direct experiments, made with general and indeterminate views, in order to aſcertain ſomething concerning a ſubject which had given myſelf and others ſo much trouble.

I began with repeating the experiments in which I had found that inflammable air, made red-hot in flint glaſs tubes, gave them a black tinge, and was in a great meaſure abſorbed, which I had diſcovered to be owing to the calx of lead in the glaſs attracting phlogiſton from the inflammable air. As the quantity of air in theſe tubes was very ſmall, though I gave it as my opinion, that the reſiduum in one of the proceſſes was phlogiſticated air, becauſe I perceived no marks of aſcenſion on preſenting to it the flame of a ſmall candle; I was not, on recollection, ſatisfied with this concluſion, and was deſirous of repeating the experiment with more care, eſpecially as, in one of the above-mentioned experiments, I found only a very ſmall bubble of the inflammable air in the tube in which it had been heated.

I found, however, great difficulties in repeating theſe experiments; and the quantity of inflammable air operated upon in them is neceſſarily ſo ſmall, that the reſult is always liable to much uncertainty. I thought, therefore, that throwing the focus of a burning lens upon a quantity of pounded flint glaſs, ſurrounded with inflammable air, or rather

on

on the calx of lead alone, in the same circumstances, would be a much easier experiment, and might bring me nearer to my object; and on making the experiment it immediately answered far beyond my expectation.

For this purpose, I put upon a piece of a broken crucible (which could yield no air) a quantity of minium, out of which all air had been extracted; and placing it upon a convenient stand, introduced it into a large receiver, filled with inflammable air, confined by water. As soon as the minium was dry, by means of the heat thrown upon it, I observed that it became black, and then ran in the form of perfect lead, at the same time that the air diminished at a great rate, the water ascending within the receiver. I viewed this process with the most eager and pleasing expectation of the result, having at that time no fixed opinion on the subject; and therefore I could not tell, except by actual trial, whether the air was decomposing in the process, so that some other kind of air would be left, or whether it would be absorbed *in toto*. The former I thought the more probable, as if there was any such thing as phlogiston, inflammable air, I imagined, consisted of it, and something else. However, I was then satisfied that it would be in my power to determine, in a very satisfactory manner, whether the phlogiston in inflammable air had any *base* or not, and if it had, what that base was. For seeing the metal to be actually revived, and that in a considerable quantity, at the same time that the air was diminished, I could not doubt but that the calx was actually imbibing something from the air; and from its effects in making the calx into metal, it could be no other than that to which chemists had unanimously given the name of *phlogiston*.

Before this firſt experiment was concluded, I perceived, that if the phlogiſton in inflammable air had any baſe, it muſt be very inconſiderable: for the proceſs went on till there was no more room to operate without endangering the receiver; and examining, with much anxiety, the air that remained, I found that it could not be diſtinguiſhed from that in which I began the experiment, which was air extracted from iron by oil of vitriol. I was therefore pretty well ſatisfied that this inflammable air could not contain any thing beſides phlogiſton; for at that time I reduced about 45 ounce meaſures of the air to five.

In order to aſcertain a fact of ſo much importance, with the greateſt care, I afterwards carefully expelled from a quantity of minium all the phlogiſton, and every thing elſe that could have aſſumed the form of air, by giving it a red heat when mixed with ſpirit of nitre; and immediately uſing it in the manner mentioned above, I reduced 101 ounce meaſures of inflammable air to two. To judge of its degree of inflammability, I preſented the flame of a ſmall candle to the mouth of a phial filled with it, and obſerved, that it made thirteen ſeparate exploſions, though weak ones (ſtopping the mouth of the phial with my finger after each exploſion), when freſh made inflammable air, in the ſame circumſtances, made only fourteen exploſions, though ſtronger ones.

After this experiment I could not heſitate to conclude, that this inflammable air went totally, and without decompoſition, into the lead which I formed at that time; and if the neceſſary circumſtances of the experiment be conſidered, it will be thought extraordinary that, even admitting this, the reſult ſhould be ſo deciſively clear in favour of it: for, in the firſt place, the greateſt care muſt be uſed to expel all air from the minium,

and it must be used before it can have atttacted any from the atmosphere; and in the next place, the water also (a considerable quantity of which must be used, and which will also be heated in the process) should be made as free from air as possible. In these circumstances, had I found the small residuum, of 2 ounce measures from 101, to have been phlogisticated or fixed air, I should not have been disappointed; and it would not have prevented my concluding that *phlogiston* was the same thing with *inflammable air*, contained in a combined state in metals, just as fixed air is contained in chalk and other calcareous substances; both being equally capable of being expelled again in the form of air.

Afterwards using a calx of lead, which had been prepared in the same manner with the former, but which had remained some weeks exposed to the air, I found, that when by using it I had reduced 150 ounce measures of inflammable air to 10, this residuum was phlogisticated air. But examining this calx separately, I found that it gave, by heat in a glass vessel, a considerable quantity of phlogisticated air.

I must observe, that the minium should not be reduced to a perfectly compact *glass of lead*; for then it would be too refractory to be easily revived by this process. Making use of some of it, I found that I could only melt it; but that a copious black fume came from it, and coated the inside of the receiver: an experiment which I shall repeat and re-consider. I must also observe, that the lead which I procured in the above-mentioned process was not to be distinguished from any other lead, and that the inflammable air was all procured from iron by oil of vitriol.

When I made use of inflammable air from wood, I found, that though I was able to reduce minium with it, it was effected

effected with more time and difficulty. Forty ounce meaſures of this kind of inflammable air I reduced to 25; when I found that the heat of the lens produced only *glaſs of lead*, and no *metal*. The air was ſtill, however, inflammable; and there was a ſmall mixture of fixed air in it. This kind of inflammable air, which burns with a lambent flame, I have ſome reaſon to think, conſiſts of an intimate union of fixed air with that which is of the *exploſive* kind extracted from metals. The reſult of thoſe experiments which I made with that kind of inflammable air which is collected in the proceſs for making phoſphorus, and which burns with a lambent yellow flame, was ſimilar to thoſe which I made with inflammable air from wood, which burns with a lambent white flame.

Having had this remarkable reſult with inflammable air, I immediately tried all the other kinds of air in the ſame manner; but in none of them did I procure any thing from the minium beſides glaſs of lead, except in alkaline air, and vitriolic acid air. In fixed air, nitrous air, phlogiſticated air, marine acid air, fluor acid air, as alſo in common and dephlogiſticated air, I got no metal at all. In vitriolic acid air there was but a ſmall quantity of lead produced, and I have obſerved that this kind of air imparts a certain portion of phlogiſton to common air, rendering it in ſome meaſure phlogiſticated, though by no means in ſo great a degree as nitrous air. Though nitrous air and phlogiſticated air certainly contain phlogiſton, they appear by theſe experiments to hold it too obſtinately to part with it to minium in this proceſs, though nitrous air quits it ſo readily to reſpirable air. I would obſerve, that there were ſome peculiar appearances in the experiments I made to revive the calx of lead in theſe kinds of air in which the attempt did

not ſucceed; but I muſt repeat the experiments, and note the appearances more accurately, before I report them.

In alkaline air lead ſeems to be formed from the minium as readily as in inflammable air, and indeed I thought rather more ſo; and this is a remarkable confirmation and illuſtration of theſe experiments, in which, by taking the electric ſpark in a quantity of alkaline air, I converted it into three times as much pure inflammable air; an experiment which, on account of the extraordinary nature of it, I have repeated many times ſince I firſt publiſhed the account of it, and always with the ſame reſult.

This experiment alſo throws ſome light upon thoſe in which, by ſuper-phlogiſticating iron with nitrous air, I produced a ſtrong ſmell of volatile alkali; an experiment which I have alſo frequently repeated with the ſame reſult. The reviving of lead in alkaline air may alſo help us to conceive how all *acids* ſhould have an affinity both to *phlogiſton* and to *alkalies*, which have hitherto appeared to be things ſo very different from each other; ſince, from theſe experiments, it is probable that one of them is ſome modification of the other, or a combination of ſomething elſe with the other. To trace the connection between the alkaline and inflammable principles, is a curious ſubject; and from theſe hints it may, perhaps, not be very difficult to proſecute it to advantage. It is evident, however, from the following experiments, that alkaline air is the compound and inflammable air, or phlogiſton, the more ſimple ſubſtance of the two.

From 5½ ounce meaſures of alkaline air I got, by means of litharge, 17 grains of lead, beſides ſome that was diſſolved in the mercury, by which the air was confined. There remained 2½ ounce meaſures, which appeared to be phlogiſticated air, and

and to have no fixed air in it. At another time, in eight ounce meaſures of alkaline air I got 15 grains of lead, beſides what was diſſolved in the mercury, which ſeemed to be a good deal in proportion to it. It was obſervable, that there remained in this proceſs 3½ ounce meaſures of phlogiſticated without any mixture of fixed air in it, though the maſſicot which I uſed at this time gave by heat only a good deal of pretty pure fixed air. Theſe experiments with alkaline air well deſerve to be reſumed, and I ſhall not fail to do it at a proper opportunity.

Having thus produced lead in inflammable air, I proceeded in my attempts to revive other metals from their calces by the ſame means; and I ſucceeded very well with tin, biſmuth, and ſilver; tolerably well with copper, iron, and regulus of cobalt; but not at all with regulus of antimony, regulus of arſenic, zinc, or the metal of manganeſe.

I was deſirous alſo of aſcertaining by this means the *quantity* of phlogiſton that enters into the compoſition of the ſeveral metals; but in this I found more difficulty than I had expected: and this aroſe chiefly from the allowance that was to be made for the inflammable air which entered into that part of the calx which was only partially revived; and it was not eaſy to revive the whole of any quantity of calx completely.

After many trials, I think I may venture to ſay, that an ounce of *lead* abſorbs 100 ounce meaſures of inflammable air, or perhaps ſomething more; for in one reſult it ſeemed to have imbibed in the proportion of 108 ounce meaſures.

An ounce of *tin* abſorbs inflammable air in the proportion of 377 ounce meaſures to the ounce. An ounce of copper from verditer abſorbed 403 ounce meaſures; from a ſolution of blue vitriol, precipitated by ſalt of tartar, and afterwards made red-hot with ſpirit of nitre, 640; but from blue vitriol itſelf 909

ounce measures. In this case, however, much of the inflammable air went to the formation of the vitriolic acid air, the smell of which was very perceivable in the course of the experiment. The copper that I made in this way was brittle, and therefore seemed not to be perfectly metalized; but being fuzed with borax it became perfect copper, and, as I think, without any loss of weight.

Bismuth absorbed inflammable air in the proportion of 185 ounce measures to the ounce. The calx I used was a precipitate from the solution of this metal in spirit of nitre.

Iron I got from a precipitate of a solution of green vitriol by salt of tartar, moistened with spirit of nitre, and exposed to a red heat. This calx absorbed in the proportion of 890 ounce measures of the inflammable air to an ounce of iron, which was in the form of a black powder; but to all appearance as much attracted by the magnet as iron filings. But it could not be expected, that perfect iron, containing its full proportion of phlogiston, should be produced in this manner, since inflammable air may be expelled from perfect iron in this very process.

Silver I evidently revived from a solution of it in spirit of nitre precipitated by salt of tartar, and also from *luna cornea*. A quantity of this last substance absorbed 23 ounce measures of inflammable air; but I could not get any calx of silver free from small grains of the perfect metal, which was easily discovered by a magnifier, and therefore I could not ascertain the quantity of inflammable air absorbed by it.

Small grains of regulus *cobalt* I produced from zaffre, and inflammable air was absorbed; but I did not estimate the quantity.

A quantity of *manganeſe* abſorbed 7 ounce meaſures of inflammable air; but I could not perceive any thing in it which had the appearance of metal. But I imagined I had not heat enough for the purpoſe, and mixing with it ſome calcined borax, I repeated the experiment, when there was again an evident abſorbtion of air, and in the courſe of that experiment, I once thought that I did perceive a ſmall globule of metal.

Zinc and *arſenic* were only ſublimed in this proceſs. The ſame was the caſe with the glaſs of *antimony*; but the experiment was attended with this peculiar circumſtance, that when the glaſs was melted in inflammable air it formed itſelf into needle-like cryſtals arranged in a very curious manner, though I could not produce that appearance in other kinds of air.

Inflammable air being clearly imbibed by the calces of metals, and thereby reviving them, is a ſufficient proof of its *containing* what has been called phlogiſton; and its being abſorbed by them *in toto*, without decompoſition, is a proof of its being nothing beſides *phlogiſton in the form of air*, unleſs there ſhould be ſomething ſolid depoſited from it at the ſame time that the proper phlogiſtic part of it was abſorbed. With reſpect to this, I can only ſay that, in the courſe of the experiments, I did not perceive any thing of the kind: for though in ſome of the proceſſes there was a black ſmoke produced, in others I could perceive nothing but part of the calx ſubliming, and clouding the glaſs. On this account, however, I could not pretend to aſcertain the weight of the inflammable air in the calx, ſo as to prove that it had acquired an addition of weight by being metallized, which I often attempted. But were it poſſible to procure a perfect calx, no part of which ſhould be ſublimed and diſperſed, by the heat neceſſarily to be made uſe of in the proceſs, I ſhould not doubt but that the quantity

quantity of inflammable air imbibed by it would ſufficiently add to its weight.

Beſides the formation of metals from their calces, I had other proofs, and of a nature ſufficiently curious, of inflammable air containing phlogiſton, though perhaps not ſufficiently concluſive with reſpect to its being wholly and ſimply phlogiſton itſelf. Thus, by means of it, I was able to make *phoſphorus*, *nitrous air*, *liver of ſulphur*, and *ſulphur* itſelf, in all of which phlogiſton is acknowledged to be a principal ingredient.

Throwing the focus of the lens upon a quantity of that glaſſy matter which is made from calcined bones by oil of vitriol in inflammable air, ſome of it was abſorbed, and all the inſide of the receiver was covered with an orange-coloured ſubſtance, which had a ſtrong ſmell of phoſphorus. I then wanted ſun-ſhine to continue the experiment; but I was ſatisfied that there was ſufficient proof of phoſphorus being actually formed in this manner. With alkaline air I ſucceeded much better.

In 2½ ounce meaſures of this air I produced, from the glaſſy matter mentioned above, 2 grains of phoſphorus in one maſs, the veſſel being only filled with white fumes during the proceſs. One-fourth of the bulk of the air remained, and this was inflammable, burning with a yellow lambent flame, exactly like that which is produced in the proceſs for making phoſphorus.

That nitrous air contains phlogiſton is ſufficiently evident, if there be any ſuch thing as phlogiſton: and I have farther proved, that it contains as much phlogiſton, in proportion to its bulk, as inflammable air itſelf. I have now, however, the farther ſatisfaction to be able to make nitrous air from its two conſtituent principles, *viz.* nitrous vapour and inflammable air. The moſt eaſy proceſs for this purpoſe is, to throw a

ſtream of nitrous vapour into a large phial previouſly filled with inflammable air. In this manner nitrous air is inſtantly formed, and in great quantities; but as this nitrous vapour is produced by the rapid ſolution of biſmuth in ſpirit of nitre, which at the ſame time produces a quantity of nitrous air, the experiment is not quite unexceptionable. I therefore attempted the ſame thing in the following manner.

Taking a quantity of what I have called a *nitrated calx* of lead, which I firſt produced by uniting nitrous vapour to minium (in conſequence of which, from being a red and powdery ſubſtance, it became white, compact, and brittle), I placed it upon a ſtand, in a receiver filled with inflammable air, and throwing the focus of the lens upon it, there was a diminution of the inflammable air, which amounted to about two-thirds of the whole, and during this time lead was revived from the calx. After this there was no more diminution of the air, or revival of the calx: and then examining what remained of the air, I found it to be all ſtrongly nitrous: and, from the circumſtances in which it was produced, it muſt have been formed from the nitrous vapour contained in the calx, and the inflammable air in the receiver. In order to aſcertain the purity of this nitrous air, I mixed it with an equal quantity of common air, and found that they occupied the ſpace of 1,32 meaſures. Freſh nitrous air, made in the uſual way, and mixed with common air in the ſame proportion, occupied the ſpace of 1,26. This difference aroſe not from any impurity in the nitrous air, but from the mixture of the dephlogiſticated air, which is alſo expelled from this calx by heat.

Liver of ſulphur was procured by throwing the focus of the lens upon vitriolated tartar in inflammable air, and it appeared to be perfectly well formed.

Laſtly,

Laſtly, to produce *ſulphur*, I threw the focus of the lens on a quantity of oil of vitriol, contained in an hollow earthen veſſel, and evaporated it to dryneſs in a receiver filled with inflammable air, in conſequence of which the inſide of the receiver acquired a whitiſh incruſtation, which when warmed had a ſtrong ſmell of ſulphur; and repeating the proceſs in the ſame receiver, I was able, this ſecond time, to ſcrape off enough of the matter to put on a piece of hot iron, and to produce the genuine blue flame, as well as the peculiar ſmell of ſulphur.

I ſhall conclude theſe obſervations on phlogiſton with two articles; one of which ſeems to contradict an eſtabliſhed maxim among chemiſts; and the other a former opinion of my own.

It is generally ſaid, that charcoal is indeſtructible, except by a red heat in contact with air. But I find that it is perfectly deſtructible, or decompoſed, *in vacuo*, and by the heat of a burning lens almoſt wholly converted into inflammable air; ſo that nothing remains beſides an exceedingly ſmall quantity of white aſhes, which are ſeldom viſible, except when, in very ſmall particles, they happen to croſs the ſun-beam, as they fly about within the receiver. It would be impoſſible to collect or weigh them; but, according to appearance, the aſhes thus produced from many pounds of wood could not be ſuppoſed to weigh a grain. The great weight of aſhes produced by burning wood in the open air ariſes from what is attracted by them from the air. The air which I get in this manner is wholly inflammable, without the leaſt particle of fixed air in it. But, in order to this, the charcoal muſt be perfectly well made, or with ſuch a heat as would expel all the fixed air which the wood contains; and it muſt be continued till it yield inflammable air only, which, in an earthen retort, is ſoon produced.

Wood,

Wood, or charcoal, is even perfectly deſtructible, that is, reſolvable into inflammable air, in a good earthern retort, and a fire that would about melt iron. In theſe circumſtances, after all the fixed air had come over, I have ſeveral times continued the proceſs during a whole day, in all which time inflammable air has been produced equally, and without any appearance of a termination. Nor did I wonder at this, after ſeeing it wholly vaniſh into inflammable air *in vacuo.* A quantity of charcoal made from oak, and weighing about an ounce, generally gave me about five ounce meaſures of inflammable air in twelve minutes.

The ſecond article that I ſhall now mention affords an indiſputable proof of the generation of fixed air from dephlogiſticated air and phlogiſton, or inflammable air. I have ſeveral times given it as my opinion, that fixed air is a *factitious ſubſtance*, and a modification of the nitrous and vitriolic acids, my former experiments greatly favouring that concluſion; but that it was compoſed of dephlogiſticated air and phlogiſton, though maintained by my friend Mr. KIRWAN, I was far from being ſatisfied with, till I was forced to conſent to his proof of it from my own former experiments, and gave him leave to mention it, as he has done in his late excellent paper on ſalts. But I have lately had two direct proofs of it by experiment.

The firſt was when, in repeating a beautiful experiment firſt made by Dr. INGEN-HOUSZ, but with ſome variation, I was firing ſome ſhavings of iron in dephlogiſticated air confined by mercury, by means of a burning lens. In this way I quickly fired the iron, and it burned away in a very pleaſing manner. But what ſtruck me moſt was, that, of the air that remained, a conſiderable portion was fixed air, though in the receiver I had nothing but the pureſt dephlogiſticated air, together with the

iron, which could only give inflammable air. I would obſerve, that the melted iron formed itſelf into large balls, which appeared to be a mere *ſlag* or *glaſs*, and was no longer iron.

Afterwards, to put this hypotheſis concerning the conſtituent principles of fixed air to a more direct proof, I mixed iron filings, which gave only inflammable air, with red precipitate, which I found to give nothing but the pureſt dephlogiſticated; and when I heated them in a coated glaſs retort, they gave a great quantity of fixed air, in ſome portions of which nineteen-twentieths were abſorbed by lime-water; but the reſiduum was inflammable. However, when I mixed with iron filings a quantity of powdered charcoal, which I had found to give only inflammable air, the fixed air produced from it was ſo pure, that only one-fortieth part of it remained unabſorbed by water; ſo that this fixed air was as pure as that which is generally procured from chalk by oil of vitriol.

It appeared, in ſome of theſe experiments, that three ounce meaſures of dephlogiſticated air go into the compoſition of two ounce meaſures of fixed air. For one ounce of this red precipitate gave 60 ounce meaſures of dephlogiſtated air; and when mixed with two ounces of iron filings, it gave about 40 ounce meaſures of fixed air that were actually abſorbed by water, beſides a reſiduum that was inflammable. I had the ſame proportion when I uſed half an ounce of each of the materials. But when I uſed one ounce of each, I got only 20 ounce meaſures of fixed air, including the reſiduum. At other times I had different proportions with different quantities of iron filings and charcoal.

I cannot conclude theſe obſervations without taking notice, how very valuable an inſtrument in philoſophy is a good burning lens. This muſt have been perceived in many of my former

former experiments, but more eſpecially in theſe. By no other means can heat be given to ſubſtances *in vacuo*, or in any other kind of air beſides atmoſpherical; and without ſome method of doing this, no ſuch experiments as theſe can poſſibly be made. I therefore congratulate all the lovers of ſcience on the ſucceſsful attempt of Mr. PARKER to execute ſo capital an inſtrument as he has done of this kind. Such ſpirited and generous exertions reflect honour on himſelf and on our country. It is only to be wiſhed, that we could have lenſes of a ſmaller ſize (*viz.* from 12 to 18 inches diameter) made tolerably cheap, ſo that they might be in more common uſe. All my experiments were made with one of 12 inches in diameter.

Experiments relating to the ſeeming Converſion of Water into Air.

SINCE many perſons have expreſſed a wiſh to be acquainted with the experiments I have lately made, which at firſt ſeemed to favour the idea of a *converſion of water into air*, but which terminated in the diſcovery of a fact, in my opinion, ſtill more extraordinary, I ſhall ſubmit to the Royal Society the reſult of the obſervations I have already made; though, as yet, I have by no means been able to ſatisfy myſelf ſo fully as I could wiſh with reſpect to ſome particulars connected with the ſubject. All the *facts* which I ſhall ſtate may be depended upon; but it is probable, that different perſons may draw different *concluſions*

from them; and to mere opinions I have never ſhewn myſelf much attached.

Having formerly obſerved ſeveral remarkable changes in fluid ſubſtances, in conſequence of long expoſure to heat in glaſs veſſels hermetically ſealed (of which an account may be ſeen in the fourth volume of my Philoſophical Obſervations); I then formed a deſign of expoſing all kinds of ſolid ſubſtances to great heats, in a ſimilar ſtate of confinement; and for that purpoſe provided myſelf with a caſt-iron veſſel, which I could cloſe at one end, like a digeſter, and of ſuch a length, that one of the ends might be red-hot, while the other was ſufficiently cool to be handled. To this end there was a cock connected to a tube, by means of which I could let off ſteam, or air, in any period of the proceſs.

I imagined, that when ſubſtances conſiſting of parts ſo volatile as to fly off before they had attained any conſiderable degree of heat, in the uſual preſſure of the atmoſphere, were compelled to bear great heats under a greater preſſure, they might aſſume new forms, and undergo remarkable changes, ſimilar to what we may ſuppoſe to be the caſe within the bowels of the earth, where, by means of ſubterraneous fires, various ſubſtances bear great heats under very great preſſures.

I have had this inſtrument ſome years; but it was ſo ill conſtructed, that I could not make the uſe of it that I had originally intended. I therefore lately fitted up ſome gun-barrels in the ſame manner, and made my firſt experiment with limeſtone; expecting, that when the fixed air, and other volatile matters, that might be contained in it, ſhould be compelled to bear a red heat, without a poſſibility of making their eſcape, the ſubſtance itſelf might undergo ſome change; but I had no particular expectation concerning the nature of that change.

I had, however, been ſo often favoured with valuable reſults from merely putting things into new ſituations, that I was encouraged to make the experiment; but I found an unexpected difficulty in getting a cock that would be air-tight and ſteam-tight under ſo great a preſſure as I wiſhed to apply.

I was mentioning theſe ideas to Mr. WATT, in whoſe neighbourhood I have the happineſs to be ſituated, when he mentioned a ſimilar idea of his, *viz.* that of the poſſibility of the converſion of water, or ſteam, into permanent air; ſaying, that ſome appearances in the working of his fire-engine had led him to expect this. He thought that if ſteam could be made red-hot, ſo that all its latent heat ſhould be converted into ſenſible heat, either this or ſome other change would probably take place in its conſtitution. The idea was new to me, and led me to attend more particularly to my former projects of a ſimilar nature, and I began with lime-ſtone, wiſhing to try the effect of giving a red heat to lime in which water only ſhould be previouſly combined, thinking it might poſſibly have the ſame effect with making the water itſelf red-hot.

Accordingly, I took a quantity of well calcined lime, and mixing with it a little water, out of which all air had been carefully boiled, I expoſed it gradually to a ſtrong heat in an earthen retort, ſuch as I had been uſually ſupplied with by Mr. WEDGWOOD (who is as much diſtinguiſhed by his love and generous encouragement of ſcience, as he is by his improvements in his own curious art), not imagining that it could make any difference whether the lime, ſo prepared, ſhould receive its heat in an earthen retort, or in a veſſel of iron or glaſs. Proceeding, however, in this manner, I found that nothing came over in the form of *ſteam*, but that there was a great quantity of *air*, ſeveral hundred times more than the bulk of the water, and at

that time there was in it a conſiderable proportion of fixed air, which I imagined might either be that which had not been ſufficiently expelled before, or might be compoſed of ſome phlogiſtic matter contained in the lime, and the purer air that was yielded by the water: for I own I then concluded, that the air which I got (and which, when the fixed air was extracted from it, was ſuch as a candle would juſt burn in) came from the water, eſpecially as in ſome of the proceſſes, the weight of the air I caught was very nearly, if not quite equal to that of the water, and interpoſing a large glaſs balloon between the retort and the recipient for the air, I obſerved that it remained perfectly cool and dry during the whole proceſs; and ſeveral hours afterwards there was not the leaſt moiſture condenſed in it. I alſo received a quantity of another produce of air made in this manner in mercury, and having viewed it with the greateſt attention, obſerved that, after ſeveral days, it never depoſited the leaſt moiſture.

I then calcined a quantity of natural lime-ſtone with this glaſs balloon, interpoſed in the ſame manner, and found no water, but only air to come from it, though the ſtone is generally ſuppoſed to contain water. But when I uſed much more than half an ounce of water to the quantity of whiting or lime above-mentioned, I always had ſome water come over, though very little in proportion to the quantity made uſe of.

I did not fail to examine whether there had been any loſs in the weight of the lime, or whiting, in order to determine whether any part of theſe ſolid ſubſtances had entered into the compoſition of the air; but I found much difficulty in weighing them with exactneſs, after ſhaking them out of an earthen retort, into which I could not ſee, and to which part of theſe earthy matters often adhered, ſo that I could not obtain much ſatisfaction even when I broke the retort. Beſides, there was

always ſome loſs of the earth in the cloudineſs of the air, whenever the production of it was rapid. In a future proceſs I had abundant proof that the air did not come from any earthy matter with which the water had been combined.

Hitherto I had no idea but that all that was neceſſary to the converſion, as I concluded it to be, of water into air, was to give it a red heat, without which it would not quit the calcareous earth; and I imagined by this means the matter or principle of *heat* was ſo intimately combined with it, as not to be ſeparated from it by cooling, as in the caſe of ſteam. But I, as well as all my friends, was a long time utterly diſconcerted upon finding that when I put the whiting and water into a coated glaſs retort, the water came over in the form of ſteam, and little or no air was produced. The reſult was alſo the ſame when I made the proceſs in a gun-barrel, in a porcelain retort, or even in an earthen retort glazed in the inſide.

That the earth had not loſt its property of doing its part in the buſineſs, I found by putting more water to the ſame whiting which had failed in the glaſs retort, and which had been uſed no leſs than four times before, and then heating it in an earthen retort; when again it gave air only, and no water, the ſame as before. And at this time I obſerved, that part of the air was hardly to be diſtinguiſhed from that of the atmoſphere.

I cannot expreſs my ſurpriſe at my unexpected failure with the glaſs retort; and my ſpeculations on the ſubject were various, but at that time altogether ineffectual. Among other things it occurred to me that, poſſibly, ſome phlogiſton, either contained in the earthen retort, or coming through it (though I could not tell how, or on what principle) from the fire, might be neceſſary to water, and all other ſubſtances, aſſuming the form of air. But when, with this idea, I put ſpirit of wine, oil,

oil, or iron-filings to the lime, I got nothing from theſe mixtures in glaſs retorts beſides ſteam and inflammable air, from the decompoſition of theſe ſubſtances containing phlogiſton.

That there was nothing in the materials of which the earthern retort was made that neceſſarily produced the air, was evident from my not ſucceeding when I pounded a broken retort, and heated it, mixed with water, in one of glaſs.

Being ſatisfied that the production of air depended very much upon the retort itſelf, I thought of uſing the retort only with water, but without any lime, or earthy ſubſtance; and I found it ſucceed far beyond my expectation. For when I put a ſmall quantity of water into one of theſe retorts, and endeavoured to diſtil it gently, I never failed to procure about an hundred ounce meaſures of air; and this I could do as often as I pleaſed, with the ſame retort, and without its loſing any weight; and the air produced in this manner had never any portion of fixed air in it, and was always but very little inferior to that of the atmoſphere.

In all theſe proceſſes I obſerved, that very little of this air was procured till all the water that could be poured out of the retort was evaporated, for the difference in the produce was very little, whether I expoſed the retort to the fire quite full of water, or with only about an ounce meaſure of water in it, or even after letting it remain full for a ſhort time, and then pouring out all that I could from it; ſo that it was only that water which was entangled, as it were, in the pores of the retort, and which had been in ſome meaſure united to the ſubſtance of it, that had contributed to this production of air.

Theſe retorts (which Mr. WEDGEWOOD informs me are made of a mixture of freſh and of burnt Devonſhire pipe clay) are pervious to water, though not to air; ſo that while the air is produced

produced from that water which has entered the pores, the reſt is ſometimes viſibly making its eſcape in the form of a copious ſmoke on the outſide. It was evidently impoſſible, however, and contrary to all the laws of hydroſtatics, that air ſhould enter by the ſame pores by which the water or ſteam was eſcaping, and at the ſame time that its endeavour to force its way out of the retort was ſuch that it overcame a conſiderable reſiſtance from the column of water, at the mouth of my recipients. Air might have *eſcaped* through any unobſerved pores in the retort, but none could have *entered* that way: and if there was the leaſt ſenſible crack in any part of the retort, I was never able to collect any air at all.

But the following experiments may, perhaps, ſhew that it is ſufficient for the production of air that ſteam come into contact with clay ſufficiently heated. Between a copper ſtill and the glaſs tube communicating with my recipient for air, I introduced the ſtem of a tobacco-pipe; and by means of a ſmall furnace, I kept about three inches of the middle part of it moderately red-hot. In this ſtate, making the water boil, I uniformly received air, though mixed with ſteam, at the rate of five ounce meaſures in twelve minutes for more than an hour; but when I let the pipe cool, nothing but ſteam was delivered by it without any air at all. There was no fixed air in this produce, and it was all ſuch as a candle would hardly have burned it. It might, I thought, have been better and alſo more in quantity if I had not uſed the ſtem of a foul pipe. But when I uſed a clean pipe in the ſame manner, I did not find the air much, if at all improved. Suſpecting this to ariſe from the near contact of the fuel, I incloſed the tobacco-pipe in an earthen tube, and then I had air as good as I had generally

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got in the earthen retort, and not much worſe than that of the atmoſphere.

Another circumſtance I obſerved was, that if the outſide of the veſſel which contained the water or ſteam, through which it paſſed, when the requiſite heat was applied to it, was not dry, or perhaps ſurrounded with good air (for in thoſe circumſtances the following experiment differs from the preceding ones) the experiment did not ſucceed.

When I put the ball of an earthern retort, filled with moiſt clay, into an iron digeſter, and applied heat to it, I get only a very little fixed air, which was probably compoſed of a ſmall quantity of air beginning to be produced from the materials and inflammable air from the veſſel. All that come over beſides was ſteam, and at laſt inflammable air, from the veſſel itſelf.

Being now able to procure air by means of water in this moſt ſimple method, *viz.* by water only in the earthen retort, I had an opportunity of aſcertaining, with great eaſe and exactneſs, ſeveral circumſtances relating to the proceſs, and of obviating, as I thought, ſome objections to the concluſion that I had drawn from it. Among other things I fully ſatisfied myſelf that the *earth of the retort* contributed nothing at all to this production of air, but the *water only*: for having uſed the ſame retort till I had got from it nearly an ounce weight of air, or 800 ounce meaſures, I found that it had not loſt ſo much as a ſingle grain in weight. After the firſt proceſs it weighed juſt three grains more than it did at firſt, and it continued to weigh the ſame till after the laſt proceſs. This ſmall addition of weight might eaſily have come from a little of the water having been imbibed by the neck of the retort, where the heat of the fire could not reach it. When all the proceſſes were over,

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I kept the whole retort in a red heat for ſeveral hours, and then found that, beſides loſing thoſe three grains, it weighed eight grains leſs than it did at firſt.

Before this I had found, that the calcined whiting which I had uſed in the firſt experiment could not, as ſome ſuppoſed, attract from the atmoſphere any conſiderable part of the air which I got from it, after combining water with it: for two ounces of the whiting (which was the quantity which I generally made uſe of) did not attract more than eight grains of any thing when it was expoſed a whole day in an open diſh, though it had loſt more than half its weight in calcination.

It has been imagined by ſome, that the air which I got in theſe earthen retorts was that which had been attracted from the atmoſphere by the inſide ſurface of them. But, beſides that no air could ever be produced without water, to obviate this objection more particularly, when one of theſe retorts was giving its laſt portion of air, I immerſed the mouth of it in a baſon of water; and letting it cool in that ſituation, filled it again without admitting any acceſs of air to the inſide; and yet, on repeating the proceſs with it, the air was produced juſt as freely as before. This operation I repeated ſeveral times. If it be ſaid, that the outſide of the retort attracted the air, ſtill the inſide, being compoſed of the ſame materials, muſt have attracted air alſo; and it would have appeared by the aſcent of the water from the baſon, the retort being ſufficiently impervious to air.

By ſome it was imagined, that either the air itſelf that I procured, or at leaſt the power of the retort to contribute to the production of it, was owing to ſomething that was tranſmitted from the burning coals, but which could not paſs through glaſs or metals. To determine this, I took an earthen

tube, of the ſame compoſition with the retort, and putting a little water in it, placed it, ſurrounded with ſand, in a glaſs veſſel, and this again, ſurrounded alſo with ſand, in an iron one; and yet the heat tranſmitted through all theſe ſubſtances enabled the earthen tube to give air, in the ſame proportion, and of the ſame quality, as it would have done if it had been expoſed to the naked fire.

Having now procured air, by means of a water, in a very ſimple and, as I thought, an unexceptionable manner, I wiſhed to make it in greater quantities in proportion to the water employed; and for this purpoſe I firſt thought of increaſing the ſize or the thickneſs of the porous retorts; but I thought it might anſwer as well if I put into the retort, in powder, the materials of which they were made, or other ſubſtances of the ſame kind.

Accordingly, by mixing ground flint and clay in various proportions, I preſently increaſed the quantity of air much beyond my expectation. In the firſt trials, in which I had much flint and a little clay, I never failed to get 200 ounce meaſures of air from one of water. Then, uſing more clay and leſs flint, I had ſtill more air; and at laſt, leaving out the flint altogether, and uſing clay only, I never failed to get much more than 400, and generally between 500 and 600, ounce meaſures of air from one of water, which was about three-fourths of the weight of the water; and in one particular proceſs I procured very little leſs than nine-tenths of the weight of the water in air, and this air was never much leſs pure than that of the atmoſphere. Sometimes it could not be diſtinguiſhed at all from it at all by the teſt of nitrous air; and once or twice I thought it even purer than that of the atmoſphere.

I muſt here obſerve, that I found it not convenient to put ſo much water to any quantity of clay as would make it cohere in one maſs, but only ſo much as that it ſhould remain in the

form of powder. By this means it would eaſily pour out of the retort when the experiment was over.

The weight of the water expended in this production of air I aſcertained, in the moſt unexceptionable manner, by weighing the retort, with all its contents, before and after the proceſs. I ſhall explain this by the reſult of two of the proceſſes. In one of them, the retort and moiſtened clay together loſt in weight 1 oz. 4 dwts. 12 grs. after yielding 741 ounce meaſures of air, which (in the proportion of ſix grains to one ounce meaſure) would have weighed 18 dwts. 12 grs. and conſequently three-fourths of the weight of the water.

In the other proceſs the loſs of weight was 15 dwts. 18 grs. after yielding 556 ounce meaſures of air, which would have weighed 13 dwts. 21 grs. The proportion, therefore, between the weight of the air and that of the water was 111 to 116, or nearly nine to ten.

I alſo found now, that ſo much heat as I had hitherto applied was neither neceſſary nor uſeful. In the laſt mentioned proceſs the retort was conſtantly ſuſpended about ſix inches above a moderate charcoal fire; at another time more than twelve or fifteen inches above it, where a FAHRENHEIT's thermometer did not ſhew more than 210°. With this moderate heat I got 465 ounce meaſures in the courſe of about twelve hours. When the retort was ſuſpended within ſix inches of the fire, the air was generally produced at the rate of 30 ounce meaſures in five minutes. But a thermometer, the bulb of which was immerſed in the clay, was ſtill only at the heat of boiling water.

In all theſe proceſſes, however, there was evidently ſome loſs of water; for, excepting the firſt experiment with the lime, I never got the whole weight of the water in air; and it

it might be ſaid that I only expelled the air before contained in the water, though from theſe experiments it appeared to contain much more air than it had been thought capable of containing. To obviate this objection, I contrived to catch all the water that eſcaped through the pores of the retort in the following manner.

Having put the moiſtened clay in an earthen tube, to which I had fitted a cock and a long glaſs tube (by means of which I could collect all the air that came from it), I put this within an iron tube, which was cloſed at the end next the fire, but open at the other end, and ſo long that I could eaſily keep this open and quite cool while the other was in the fire; conſequently, whatever water eſcaped through the pores of the earthen tube, it would be condenſed in the cool part of the iron one. This water I carefully collected, and always found that the weight of it, together with that of the air produced in the experiment, was nearly that of the original weight of the water, eſtimated by the loſs of weight in the earthen tube and its contents. I alſo found, that the water ſo collected ſerved for the production of more air, juſt as well as any other water whatever, ſo that there had been no decompoſition of the water in the caſe.

In the laſt proceſs that I went through of this kind, the loſs of weight in the earthen tube, or rather of the water contained in it, was 12 dwts. 4 grs.; the air collected was 173 ounce meaſures, which would have weighed 4 dwts. 3 grs. and the water which eſcaped through the pores of the earthen tube, and which I collected, was nearly 8 dwts. 3 grs.; ſo that the air and this water together weighed 12 dwts. 14 grs. or ten grains more than the original water. But as I eſtimated the weight of the water only by the ſpace which it occupied in a

cylindrical glaſs tube, divided according to ounces and parts of ounces of water, it was not eaſy to avoid an error of a few grains. At other times there was an error of a ſmall magnitude on the other ſide. But it will appear hereafter, that more ſteam muſt have eſcaped inviſibly at the open mouth of the iron tube than I was aware of.

That nothing could enter by the pores of the retort at the ſame time that the water was making its eſcape out of them, I thought I aſcertained pretty ſatisfactorily by immerſing the bulb of it in mercury, contained in an iron veſſel. In theſe circumſtances I obtained air as uſual, only the produce was not ſo rapid. In this way, however, I procured above an hundred ounce meaſures of air from moiſtened clay; and I diſcontinued the proceſs without perceiving any termination of it. But the moment the retort was raiſed out of the mercury, it gave air three times as faſt as it had done before. The quality of the air was the ſame in both caſes, *viz.* a little worſe than that of the atmoſphere.

I even collected thirty ounce meaſures of air when the bulb of the ſame retort was immerſed in hot linſeed oil, but the production of air gradually ceaſed, and the next day I found the retort almoſt full of the oil, which had ſoaked through it. Diſtilling this oil I get 300 ounce meaſures of air, wholly inflammable, except a very few ounce meaſures at the laſt, which were only phlogiſticated.

Still hearing of many objections to the converſion of water into air, I now gave particular attention to an experiment of Mr. CAVENDISH's, concerning the re-converſion of air into water, by decompoſing it in conjunction with inflammable air. And in the firſt place, in order to be ſure that the water I might find in the air was really a conſtituent part of

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it, and not what it might have imbibed after its formation, I made a quantity of both dephlogiſticated and inflammable air in ſuch a manner as that neither of them ſhould ever come into contact with water, receiving them as they were produced in mercury; the former from nitre, and in the middle of the proceſs (long after the water of cryſtallization was come over), and the latter from perfectly-made charcoal. The two kinds of air thus produced I decompoſed by firing them together by the electric exploſion, and found a manifeſt depoſition of water, and to appearance in the ſame quantity as if both the kinds of air had been previouſly confined by water.

In order to judge more accurately of the quantity of water ſo depoſited, and to compare it with the weight of the air decompoſed, I carefully weighed a piece of filtering paper, and then having wiped with it all the inſide of the glaſs veſſel in which the air had been decompoſed, weighed it again, and I always found, as nearly as I could judge, the weight of the decompoſed air in the moiſture acquired by the paper.

As there is a ſource of deception in this experiment, in the ſmall globules of mercury, which are apt to adhere to the inſide of the glaſs veſſel, and to be taken up by the paper with which it is wiped, I ſometimes weighed the paper with the moiſture and the mercury adhering to it; and then expoſing it in a warm place, where the water would evaporate, but not the mercury, weighed it again, and ſtill found, as nearly as I could pretend to weigh ſo ſmall a matter, a loſs of weight equal to that of the air. I wiſhed, however, to have had a nicer balance for the purpoſe: the reſult was ſuch as to afford a ſtrong preſumption that the air was re-converted into water, and therefore that the origin of it had been water.

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Another preſumption in favour of the generation of our atmoſphere from water was, that the purity of the air that I produced from it is ſo very nearly the ſame with that of the atmoſphere. And the degree of heat requiſite to produce it is no greater than may be given by the rays of the ſun in certain circumſtances. Subterraneous fires, however, would be abundantly ſufficient for the purpoſe, as it appears to be ſufficient for the converſion of water into reſpirable air, that it come into contact with clay, and perhaps many other earthy ſubſtances in the form of vapour. I muſt, however, obſerve, that when I threw the focus of a burning lens upon a quantity of moiſt clay, either *in vacuo*, or in common air, I got no air from it.

I made this experiment both with the clay expoſed in an open diſh, and alſo confined in a ſhort earthen tube. Had I then proceeded to repeat this laſt proceſs with a communication between the inſide of the earthen tube and the external air, as I then propoſed to do, but was prevented, I ſhould much ſooner have diſcovered what I did afterwards, *viz.* that there was no real converſion of water into air in this proceſs. In favour of which, however, it may not be amiſs to obſerve, that the great difficulty Mr. DE LUC and others have found in expelling all air from water, is beſt accounted for on the ſuppoſition of the generation of air from water, though in other circumſtances than thoſe that I have obſerved. I have the pleaſure to add, that Mr. DE LUC himſelf concurs with me in this opinion.

The difficulty that ſtrikes many perſons the moſt forcibly, is the want of analogy between the converſion of water into air with any other known facts in philoſophy or in nature. But admitting that this converſion is effected by the intimate union

of what is called the *principle of heat* with the water, it appears to me to be ſufficiently analogous to other changes, or rather combinations of ſubſtances. Is not the acid of nitre, and alſo that of vitriol, a thing as unlike to air as water is, their properties being as remarkably different? And yet it is demonſtrable, that the acid of nitre is convertible into the pureſt reſpirable air, and probably by the union of the ſame principle of heat.

It is true, that ſteam is a thing very different from air, and I find that it is not able to decompoſe nitrous air; but then, though it has acquired ſenſible heat, it has got no latent heat ſo intimately combined with it as it is with air; and for the ſame reaſon, perhaps, the vapour of nitrous acid is not dephlogiſticated air.

By the ſame proceſs by which reſpirable air is made by means of water, inflammable air may be made from liquid ſubſtances containing phlogiſton. Making ſpirit of wine to boil in a glaſs retort, I made the vapour paſs through the ſtem of a hot tobacco-pipe, and found that it was all ſeemingly converted into inflammable air, and it was of that kind which burns with a lambent white flame. But when I let the pipe cool no air was produced, but only vapour, which was inſtantly condenſed in the water.

Being now maſter of a new and eaſy proceſs, I was willing to extend it to other liquid ſubſtances; and I preſently found, as I then imagined, that, by this means, I could give a permanent aërial form to any liquid ſubſtance that had been previouſly thrown into the form of vapour.

When I made the vapour of ſpirit of nitre, heated in a glaſs retort, paſs through the ſtem of the hot tobacco-pipe, I got as pure dephlogiſticated air as ever I have procured from

nitre; though the cork, by which the retort was connected with the pipe, was diſſolved, and muſt have contributed to contaminate it, and give it a ſlight mixture of fixed air.

With oil of vitriol I got air conſiderably phlogiſticated, ſo that a candle would not have burned in it; but this I alſo attribute to the cork, which was diſſolved in the proceſs. The reſult was nearly the ſame when I uſed water impregnated with vitriolic acid air, though the cork was not diſſolved. But this acid is known to contain much phlogiſton.

Spirit of ſalt gave air no purer than the beſt atmoſpherical air. But as by this proceſs I never got air ſo pure as this from water only, I concluded, that even this acid, as well as the nitrous and vitriolic, is capable of being turned into dephlogiſticated air.

When I uſed water impregnated with fixed air, this air was expelled by the heat, and came over without any change that I could perceive, except that the reſiduum was larger, from the water that came along with it. The air I got afterwards was only that from the water, and of the ſame quality as if it had not been impregnated with fixed air.

Water impregnated with alkaline air gave neither fixed nor inflammable air, which I had rather expected, but only air conſiderably phlogiſticated; though ſome of it was ſo pure that a candle would have burned in it.

N. B. In all theſe experiments with the tobacco-pipe all the air was remarkably turbid, like milk, and even the common air in the retort before the proceſs properly began.

In this ſtate of the experiments I think I may venture to ſay, that no perſon could have ſeen them without concluding that there was a real converſion of water into air, there being no known principle or fact in philoſophy, that could have led

any person to suspect a fallacy in the case. In this, therefore, I must have acquiesced, as indeed did all my acquaintance, even those who had been the most incredulous on the subject, after they had themselves seen the experiments. But I was led to the farther prosecution of this business, in consequence of having observed that the *purity* of the air which I procured depended upon the state of that which was immediately contiguous to the earthen retort, or tube, in which I supposed the conversion to have been made; and that some communication with the atmosphere was necessary to the production of any air, as in the experiment with the digester, and those with the clay and the burning lens. And since pure external air was necessary in order to procure good air, it was concluded by several of my friends, and especially Mr. WATT, that the operation of the earthen retort was, to transmit phlogiston from the water contained in the clay to the external air; and that the water, thus dephlogisticated, was capable of being converted into respirable air by the intimate union of the principle of heat.

In order to ascertain what the influence of the external air in this case really was, I inclosed an earthen retort filled with moistened clay in a large glass receiver, open at both ends, through the upper orifice of which (being narrow) I thrust the neck of the retort, luting it so as to be perfectly air-tight; and placing the receiver in a bason of water, by which the air within was cut off from all communication with the external air, I fitted to the mouth of the retort a glass tube, through which I could receive whatever was produced in the process. In this situation I heated the retort by means of Mr. PARKER's excellent burning lens, when air was received through the tube communicating with the inside of the retort as usual; but at the same

time the water roſe within the receiver. This effect might be owing to a phlogiſtication of the air within the receiver; but it was ſoon diminiſhed far beyond the utmoſt limit of that proceſs, ſo that very little of it remained; and examining this air, I found it to be but very little worſe than that of the atmoſphere, as that which came from the retort was a little better.

This experiment made it probable, that the air on the outſide of the receiver had actually paſſed through it, only a little purified in its paſſage; and yet it was contrary to all the known principles of hydroſtatics, and even any thing hitherto known in chemiſtry, that air ſhould be tranſmitted through a veſſel of this kind, and in a direction contrary to that in which it would have been forced by the preſſure of the atmoſphere; while the water, with which the clay was moiſtened, went the other way. For had the retort been pervious to air, as the inſide had a free communication with the atmoſphere, the water could not have riſen within the receiver. This, however, appeared to be the caſe by the following deciſive experiments.

Having filled the earthen retort with the moiſtened clay as before, I made the inſide of the receiver perfectly dry, and placed it in a baſon of mercury; when, upon heating the retort as before, the receiver was all covered with dew, which collecting into drops trickled down the inſide of the receiver, and remained upon the mercury, which roſe within the receiver, while air was received from the retort as uſual. I had no doubt, therefore, but that all the water within the retort would have got through into the receiver. Spirit of wine, or ſomething that had the ſmell of it, was tranſmitted from the clay through the retort in the ſame manner.

I then filled the receiver with inflammable air, and upon heating the retort it was all drawn through it, and delivered

as ſtrongly inflammable as ever by the tube communicating with the inſide of it, while the water roſe within the receiver, and even covered the retort, which was fixed at the very top of it, ſo that hardly any of the inflammable air remained within it. In like manner nitrous air paſſed through the retort unchanged.

From theſe experiments it is impoſſible not to infer, that the clay of the earthen retort, being thus heated, deſtroys for a time the aërial form of whatever air is expoſed to the outſide of it; which aërial form it recovers after it has been tranſmitted in combination from one part of the clay to another, till it has reached the inſide of the retort, while the water is drawn through it in the contrary direction.

Had this hypotheſis been propoſed *a priori*, it would, I doubt not, have been thought more extraordinary than the converſion of water into air. I propoſe to make many other experiments in the proſecution of theſe; but till I have an opportunity of doing this, I ſhall not trouble the Society with any conjectures that have occurred to me on the ſubject.

The great difficulty with reſpect to the experiment with the lens is, that the water ſhould paſs through the retort one way, and the air the other, and yet that the air ſhould not be able to paſs without the water. It is alſo not a little extraordinary, that the weight of the air and that of the water ſhould be ſo nearly equal.

In the laſt place I muſt obſerve, that there is nothing in this experiment that contradicts the idea of the converſion of water into air, though it does not prove it: for ſtill the experiment with the tobacco-pipe, in which the ſteam is made red-hot (whereas in that with the lens it is only of a boiling heat) cannot be explained ſo well on any other hypotheſis any more

than Mr. CAVENDISH's experiment on finding water on the decompoſition of air.

I cannot conclude this account without acknowledging my obligation to Mr. PARKER for the uſe of his incomparable lens, and his obligingly aſſiſting me in the management of it. Indeed, without this very inſtrument, or one of greater power than my own, I do not know that the laſt mentioned experiments could have been made at all; certainly not to ſo much ſatisfaction.

XXIII. *Description of an improved Air-Pump, and the Account of some Experiments made with it. By Mr.* Tiberius Cavallo, *F. R. S.*

Read July 3, 1783.

THE principal improvements which the air-pump received since it was first invented, were contrived by Mr. SMEATON, F. R. S. and are described in the XLVIIth volume of the Philosophical Transactions. This ingenious gentleman, considering the imperfections of the air-pumps usually made, not only found means to correct several of them, but improved almost every part of the machine, so as to render it incomparably superior to any thing of the kind done before.

It appears, by some accurate experiments of Mr. NAIRNE, F. R. S. described in the LXVIIth volume of the Phil. Trans. which were made with an air-pump constructed after Mr. SMEATON's principle, that by means of the best air-pumps made before Mr. SMEATON's invention, the rarefaction of the air within the receiver could never have been brought to more than forty or fifty times, if the heat of the place was about 57°; that even with Mr. SMEATON's pump the receiver could not be exhausted beyond 70° or 80° of rarefaction, when moist leathers were used, or moisture was in any way introduced within the receiver; but that when this pump is quite free from moisture, and is newly cleaned, oiled, and put together,

then

then the air may by it be rarefied about 600 times, and not farther *.

The principal cauſe which prevents this pump to exhauſt farther than that limit is the weakened elaſticity of the air remaining within the receiver, which, decreaſing in proportion as the quantity of the air within the receiver is diminiſhed, becomes at laſt incapable of lifting up the valve, which opens the communication between the receiver and the barrel; conſequently no more air can in that caſe paſs from the former to the latter.

To remove this principal imperfection of the beſt air-pumps had been attempted by ſeveral ingenious perſons; but, as far as I know, was never obtained before the happy contrivance of the air-pump, which I am going to deſcribe in the following pages.

Being in want of a good air-pump, and imagining that the opening and ſhutting the communication between the barrel and the receiver might, inſtead of the valve, be performed by means of a ſtop-cock, particularly conſtructed upon an idea of mine; I communicated my plan, about the latter end of the laſt year, to Mr. JACOB BARNARD HAAS, an ingenious workman in the philoſophical-inſtrument way, who, in partnerſhip with Mr. JOHN HENRY HURTER, had lately eſtabliſhed a manufactory of philoſophical inſtruments. Mr. HAAS remarked, that according to my plan the friction of the parts of

* The degree of rarefaction ſhewed by what is called the pear-gage, when any vapour of water is within the receiver, is not to be conſidered as the degree of rarefaction of the elaſtic fluid in the receiver, but only of the air; for though the air may be exhauſted, yet the vapour of water will ſupply its place; we ſhall, therefore, only take notice of the exhauſtion when no vapour or moiſture is within the receiver. See NAIRNE's Experiments, Phil. Tranſ. vol. LXVII.

the machine would be too great, and therefore the pump, if it anſwered at all, was not likely to be durable. In conſequence of this, he conſidered upon ſome plan or other which might anſwer the ſame purpoſe in a better way, and ſoon hit upon a method of lifting up the valve at the bottom of the barrel, and of ſhutting it again at pleaſure; which method being put into execution, has been found to anſwer exceedingly well.

Beſides this capital improvement, his air-pump is rendered altogether more convenient for philoſophical experiments, by anſwering ſeveral purpoſes, which will be enumerated after its deſcription.

Plate I. ſhews a perſpective view of this pump. Plate II. exhibits a geometrical delineation of its metal or working parts detached from the wooden frame. And plate III. repreſents a ſection of its parts. The letters of reference are made to anſwer to the ſame parts in all the plates.

In plate III. fig. 1. AABI is the barrel of the pump, to the brood or flat ends of which are ſcrewed, by means of five ſcrews, the pieces CE and K, with leathers between, to render the junctures perfectly air-tight *. The upper piece CE contains the collar of leathers for the handle or axis GG of the piſton to go through, and the baſin F, which ſerves for a cap to ſcrew down the leathers, at the ſame time that it holds the oil, which may be put into it, in order to let the collar of leathers hold very tight; though it is ſhewn, by experience, that when the leathers are perfectly ſoaked, there is not the leaſt occaſion to keep any oil in the baſin F. The ſame upper piece

* All the leathers uſed for this pump are ſoaked in melted hog's lard; and when the parts of the inſtrument are put together, a little oil is ſmeared upon thoſe ſurfaces of the braſs pieces which go againſt the leather, though this oil may be ſpared.

CE

CE contains the valve at E, which lets the air paſs upwards, but prevents its return, and which is ſo contrived as that, when the piſton is drawn quite to the top of the barrel, the leaſt poſſible quantity of air ſhould be left into the barrel. The parts which form this valve are ſhewn ſeparately in fig. 3. where 1,3 is a braſs piece that ſcrews into a proper cavity made for its reception in the piece CE, and which is hollow, except its lower part, where it conſiſts of a thin lamina perforated with a ſmall hole 3. Into the hollow of the laſt mentioned part is ſcrewed the other perforated piece 2,4, having a ſlip of oil-ſilk ſtretched over its lower part 4, and tied round a ſmall indenture or groove made on its lower part. This ſlip of oil-ſilk anſwers better than a piece of bladder or leather: it juſt covers the hole 3, and is about four times broader than the diameter of the hole.

It will be eaſily conceived, that when the air is forced through the hole 3, it will lift up the ſlip of oil-ſilk, and paſſing by the ſides of it, and alſo through the large perforation of the piece 2,4, will go upwards, &c.; but can by no means return backwards, ſince any preſſure, that the air makes on the upper part of the oil ſilk, will only ſtop the paſſage more effectually.

A valve much like this is in the piſton, the parts of which are ſhewn ſeparately in fig. 4. *u* is a perforated braſs piece ſcrewed to the cylindrical handle or axis GG, which is alſo perforated with a ſhort and bent hole. The piece *x* is ſcrewed into the part *u*, and contains a valve, *viz.* a ſmall piece 6 with a ſlip of oil-ſilk tied round its groove *yy*, which ſlip of oil-ſilk bears againſt the hole 5. The piece *x* ſcrewing into the other piece *u*, faſtens the round leathers which, about thirty in number, form the ſtopping part of the piſton, and rub with their edges againſt the cavity of the barrel. This is a very uſeful improvement,

ment, ſince the common way of uſing two leathers turned over corks is both troubleſome to make, and ſeldom fits exactly.

The piece K faſtened to the lower end of the barrel is perforated with a hole, the direction of which is clearly ſeen in the figure, and which communicates with the perforation of the round piece L, which is ſcrewed to K with a leather between. The perforation therefore of K communicates with the cavity of the braſs tube RS, this being ſoldered to L. The part of the piece K, which projects within the barrel, is ſmaller in diameter than the cavity of the barrel, and the intervening ſpace is exactly fitted by the moveable ring 8, 8, the two parts of which are ſcrewed together, holding faſt between, the edge of a piece of oil-ſilk, which ſtretches over the upper part of the piece K, and covers its aperture. A vertical view of the above-mentioned oil-ſilk, with five holes in it, is ſhewn in fig. 6.

It appears from this deſcription, that the air can paſs through the valve from without to within the barrel, but not *vice verſâ*. It will be alſo eaſily conceived, that the air can paſs from the cavity of the tube R through the perforation of the pieces L and K, within the cavity of the barrel, only when the ſaid air has elaſticity, or force enough to puſh up the oil-ſilk. Now the principal improvement in this machine is, to lift up the oil-ſilk by a power applied externally, when the weakened elaſticity of the air within the cavity of the tube R, &c. is not capable of doing it by itſelf, and here follows the deſcription of this mechaniſm.

The double ring 8, 8, which holds the oil-ſilk, is faſtened to two ſteel wires 9, 9, which are ſhewn in fig. 5.; this figure being a ſection of the lower part of the pump through a plane perpendicular to the plane of the ſection, fig. 1. Thoſe wires

paſs through collars of leathers held in proper braſs boxes HQ, ſcrewed to the piece K, and furniſhed with caps, 11, 11. The lower extremities of the wires are faſtened to the croſs bar, 7, 7, of the braſs frame, OOO, a vertical view of which is ſhewn in fig. 4. of plate II.

From the middle of the piece L, fig. 1. pl. III. a pillar M proceeds, the lower part of which, branching into two horizontally, forms an axis z, z, fig. 4. pl. II. about which the braſs frame OOO moves a little way upwards and downwards.

It appears from this deſcription, that if the frame OOO fig. 4. pl. III. is moved upwards, the wires 9, 9, and likewiſe the double ring 8, 8, with the oil-ſilk, being all connected together, will be puſhed alſo upwards; conſequently, the oil-ſilk being removed from the hole of the piece K, a free communication is opened between the cavity of the tube R, and the cavity of the barrel, through which the air, however rarefied or weakened in elaſticity, can paſs without the leaſt impediment.

In order to move the braſs frame upwards, the end of a lever P bears againſt it. This lever is ſhewn in fig. 3. of plate II. which is a front view of part of the pump, whereas fig. 1. of the ſame plate is a ſide view of it. The center of motion of the lever is at 13, that is, between two ſide prominences of the piece 12, 12, which is faſtened by ſcrews to one of the wooden pillars of the frame of the machine. The part 15 of the lever, which projects beyond the wooden pillar, is made with a joint, by which means it may be turned upwards, as repreſented by 17, for packing it more conveniently.

When the valve is to be opened, the foot of the operator muſt preſs upon the extremity 15 of the lever, by which means the other extremity P, the frame OOO, the wires 9, 9, and the double ring 8, 8, with the oil-ſilk, are all lifted up. But in order

order to bring down again all thoſe parts, and to ſhut the valve when the preſſure of the foot is removed, there is an open braſs tube N, faſtened to the piece K, which contains a ſpiral ſpring, that, bearing againſt the extremity of the braſs frame OO, puſhes it downwards.

This principal mechaniſm being diſpatched, it will be very eaſy to deſcribe the remaining parts of this excellent machine: but before we proceed to that, I ſhall briefly deſcribe another mechaniſm, which Mr. HAAS has lately contrived, to ſupply the place of that juſt mentioned, *viz.* of the lower part of the inſtrument, as being much more ſimple, and capable of anſwering the ſame purpoſe.

A ſection of this new thought, which he is going to execute for another pump, is ſhewn in fig. 7. of plate III.

AB is the lower extremity of the barrel. CCDE is a piece of braſs perforated quite through with a large and cylindrical hole, and is ſcrewed to the barrel inſtead of the piece K of fig. 1. with a leather between. On one ſide of this piece CDE, part of the ſurface is flattened, and to it is adapted, by means of ſcrews and a leather, the piece of braſs G, to which the tube H is ſoldered, which correſponds to the tube RS of fig. 1. The aperture of CC, towards the cavity of the barrel, is covered by a piece of oil-ſilk, like that repreſented in fig. 6. which is kept ſtretched by a braſs ring LL, ſunk into the piece CCD.

Within the cylindrical perforation of the piece CCDE, there is a long piſton KI*r*, conſiſting of the following parts. IK is its axis, which ſpreads at top into a flat plate *r*, and the lower extremity of which is faſtened at N to the lever MO, which moves round the center M. Towards the middle of the axis, there is a piece of braſs, the ſhape of which is more eaſily

underſtood by inſpecting the figure, than by a verbal deſcription, which piece confines the round leathers from itſelf to the plate *r*, and alſo from itſelf to the other plate, which is ſcrewed upon the axis at K.

Between the laſt mentioned plate and the cap F, there is a ſpiral ſteel ſpring, which always puſhes the piſton upwards. Laſtly, the axis of the piſton is perforated from its top till towards the middle, where the perforation, opening ſide-way, communicates with the cavity of the tube H.

Now, when the piſton is puſhed upwards, as it always is when the extremity O of the lever is not preſſed down, then the oil-ſilk at LL, laying againſt the plate *r*, covers the hole of the piſton, conſequently it ſhuts the communication between the barrel and the cavity of the tube H, &c. But if this communication is required to be opened, then the extremity O of the lever is preſſed down, which will ſeparate the upper part of the piſton from the contact of the oil-ſilk, ſo as to open the communication as required.

Let us now return to the deſcription of the other parts of the machine, as ſhewn in fig. 1. The upper extremity *o* of the tube RS is made conical, and is fitted by grinding into the ſtrong braſs piece of communication UX*l*; the cap T ſerving to tighten it *. The extremity *n* of the oppoſite piece *np* is likewiſe fitted by grinding into the part *l*, and is tightened by the cap *m*. Juſt over the ſaid conical extremities *n* and *o*, and into the ſame piece of communication UX*l*, are adapted two ſtop-cocks Z and *b*, which are tightened down by the caps

* All the parts of this pump, that are fitted to each other by grinding, as the ſtop-cocks, the extremity of the tube RS, &c. before they are put together, are ſmeared with a mixture of bees-wax and oil melted together, in order to let them ſtop the better, and to prevent their wearing by friction.

Y and

Y and *i*; but as thoſe ſtop-cocks muſt be turned by means of a key adapted to their ſquare tops, and in that caſe their friction againſt the caps would unſcrew or tighten them; therefore a ring is put round each cap, which ring is prevented from turning by a pin, and is faſtened round the cap by means of a ſcrew with a milled head. Theſe rings are ſeen in fig. 1. of plate II. Each of the ſtop-cocks is perforated with a hole, which goes from one ſide to the bottom of it.

The upper part of the piece of communication terminates in a ball *a*, into which the lower and conical extremity *b* of the tube *d* is adapted by grinding, and is faſtened by the cap *c*. The tube *d* is ſoldered to the part *e*, which is made faſt to the upper board of the frame of the machine, and to which the plate *ff*, having the rim *gg*, is ſcrewed with a leather between.

The lower part of the piece *np* is ſcrewed with a leather between to the top of the ſtrong braſs veſſel *qrs*, and is terminated as appears in the figure, for a reaſon that will be made evident in the ſequel. The veſſel *qrs* is faſtened to the middle ſhelf of the wooden frame by ſcrews *ss*; and it has a perforation at bottom, which is ſhut by the ſcrew nut *t*, and ſerves to let out the oil, which, after working the machine for ſome time, will be found lodged in the veſſel *qrs*.

The tube E*r* is ſoldered into the piece D, and likewiſe into the veſſel mentioned above, wherein it proceeds till very near the top of the veſſel, where it opens.

The piſton of the pump, with its axis GG, is moved upwards and downwards by means of the rock-work, wheel, and handle; which parts being clearly ſhewn in fig. 1. and 2. of plate II. and alſo being nothing new, require no farther deſcription.

To the fore-ſide of the ball *a*, on the ſtrong piece of communication, is adapted by a croſs-piece and a ſcrew, the gage which ſhews the exhauſtion of the pump. This gage may be ſeen in fig. 1. of plate II. It conſiſts of an outward glaſs tube containing a little quickſilver; and of an inſide tube, which, like a barometer, is filled with, and inverted into, the quickſilver of the large tube. The inſide tube is ſupported at top by a ſpring ſocket. A ſmall ivory ſcale, with diviſions, encompaſſes the ſmall tube, and ſwims upon the ſurface of the quickſilver in the large tube. By means of this ſcale the exhauſtion of the pump may be begun to be meaſured, after that the air is rarefied at leaſt thirty times.

To the other ſide of the ball *a*, *viz.* oppoſite to the gage, there is a ſcrew-nut, which, by means of a leather, ſhuts a hole made in the ſaid ball, and ſerves to open the communication between the receiver and the atmoſphere. This nut, beſides a milled head, has a ſquare filed on it, to which a key may be applied, in order to open the ſcrew more eaſily, and by degrees without jerks, which can hardly be avoided when the nut is opened by one's finger applied immediately to it. This nut is ſeen in plate I.

I ſhall forbear from any more prolix deſcription of other gages that may be adapted to this pump, as alſo of other parts neceſſary for performing experiments with it, theſe things being very well known at preſent, and containing nothing new.

Now, as to the working of this pump, a bare inſpection of fig. 1. in plate III. will ſhew, that by the action of the piſton, when moved up and down into the barrel, the air will be exhauſted from the cavity of the tube RS, of the ball *a*, tube *d*, and of courſe from the glaſs receiver that is put upon the plate *ff*; for when the piſton, after being let down, is drawn upwards,

the under part of the barrel remains without any air; conſequently the valve at the bottom of the barrel, having no preſſure on one ſide, will be puſhed up by the air in the receiver, which expanding comes through the tube RS, and part of it paſſes into the barrel. Then the piſton on being let down, the air paſſes through the valve of the piſton, to the upper part of the barrel, and when afterwards the piſton is drawn up, this air is forced through the valve at E into the tube D*r*, from thence into the veſſel *qs*, through the channel *pn*, and, laſtly, it will be expelled into the atmoſphere through the aperture *k*.

As ſome ſmall quantity of oil is always neceſſary to be put into the pump, this oil, by the action of the piſton, is brought, together with the air, towards the tube D*r*, and would come out of the hole *k* if the veſſel *qs* had not been placed to receive it; and it is for this reaſon, that the lower part of the piece *pn* is ſhaped as ſhewn in the figure, and that the tube D*r* is made to proceed almoſt as far as the top of the veſſel *qs*; for if the oil was permitted to come out of the aperture *k*, it would be ſcattered about the inſtrument and the operator, by the violence of the air coming out of *k*.

As this pump exhauſts exactly in the ſame manner as other pumps do, the lever which opens the valve at the bottom of the barrel is not to be moved, except when ſuch a degree of exhauſtion is required as cannot be made by the inſtrument itſelf, *viz.* when worked in the ordinary way. In fact, it will be ſeen by the gage, that when the mercury cannot fall any lower by the uſual way of working the pump, it will be inſtantly depreſſed by opening the valve at the bottom of the barrel, which evidently ſhews the great advantage of the improvement. In general, the lever may be begun to be preſſed, or, which is the ſame thing, the valve to be opened, when the

gage

gage ſhews that the exhauſtion of the air is to about 100 times, *viz.* that the quantity of the air remaining within the receiver is about the one-hundredth part of the air that was contained in it before the exhauſtion was commenced. Care muſt be taken to open the valve only whilſt the piſton is drawing up, and to remove the foot from the lever the moment that the piſton is impelled downwards, otherwiſe the work is uſeleſs.

In the ſituation in which the pump is ſhewn by fig. 1. pl. III. it is evident, that as the action of the pump determines the air to move from the tube RS into the barrel and from the barrel up the tube D*r*, &c.; it is evident, I ſay, that if a receiver is placed upon the plate *ff*, the air will be exhauſted from it. But if the ſtop-cocks Z and *b* are ſo turned as that the ſide hole of the cock *b*, as well as that of the cock Z, is turned towards X, then, by the action of the pump, the air, inſtead of being exhauſted, is condenſed into the receiver properly placed upon the plate *ff*; for now the air coming from the atmoſphere through the aperture X goes down the cavity of the tube RS, enters the barrel, and from the barrel is impelled upwards through *Dr*, through *pn*, through *a*, *b*, *d*, and laſtly into the receiver, which in that caſe muſt be preſſed down, as is uſually done in condenſing engines. This ſituation of the ſtop-cocks, *viz.* when the inſtrument is to be uſed as a condenſer, is ſhewn in fig. 2. of plate III. Two letters, E and C, marked upon the ſquare top of each cock, direct the operator how to turn them, whether for exhauſting or for condenſing.

The conical holes X and *k*, fig. 1. plate III. are made to receive occaſionally the extremities of two ſtop-cocks. Thoſe ſtop-cocks are fitted to the holes H and *k* by grinding, and a bladder is adapted to each of them. The uſe of this contrivance is to introduce into the glaſs receiver ſome particular ſort of

of elaſtic fluid, or to receive into a bladder the elaſtic fluid that is contained in the receiver. Thus, ſuppoſe I want to introduce ſome fixed air into the receiver; firſt, I exhauſt the common air from the receiver, then put the ſtop-cock of a bladder containing fixed air to the hole X; laſtly I open the ſtop-cock of the bladder, and turn the cocks *b*Z, ſo that their ſide-holes may be turned towards X, and by working the pump the fixed air will immediately paſs from the bladder into the receiver. If now this ſame fixed air is required to be introduced into a bladder again, the ſtop-cocks muſt be turned with their ſide holes towards *k*, and a ſtop-cock with an empty bladder is put to the hole *k*; then, by working the pump, the fixed air will be gradually introduced from the receiver into the bladder.

Having finiſhed the deſcription of this improved air-pump, which, beſides its exhauſting much better, has various other advantages over any other inſtrument of the kind; I ſhall conclude this paper with the ſuccinct account of ſome experiments made with it, principally to determine how far it can rarefy the air.

Experiments made with the above-deſcribed air-pump.

Previous to the narrative of the experiments, it is neceſſary to mention, that both the plate, and the lower edges of the receivers of this pump, are ground ſo perfectly true, as not to require any leathers, nor even oil; however, for greater ſecurity, ſome oil is generally poured on the outſide of the edge of the receiver, after having exhauſted it a little; and it is very ſeldom, that any viſible quantity of this oil paſſes within the receiver, between its edge and the plate of the pump.

When the hole in the plate of the pump is ſtopped up by means of a ſcrew and leather, and the inſtrument is worked for about three or four minutes, the quickſilver in the ſmall tube of the gage falls ſo low as to be even with the quickſilver in the outſide tube, which ſhews as if the air were entirely exhauſted from the inſide of the pump; but as it is difficult to judge whether the two ſurfaces of the mercury in the inſide and outſide tube are quite on the ſame level, and even if they were exactly ſo, there could always be ſuſpected that a little air may be lodged within the gage, notwithſtanding that ſeveral gages of this ſort have been uſed, in ſome of which the mercury had been accurately boiled; we muſt, therefore, have recourſe to other gages, in order to determine the exhauſting power of this pump with more preciſion. Accordingly, the pear-gage and long barometer-gage were tried, the effects of which will be related in the ſequel.

If, inſtead of ſtopping up the hole in the plate of the pump, a glaſs receiver is laid upon it, and the pump is worked, the gage will alſo come ſo low as when no receiver is put upon it; but it muſt be remarked, that after exhauſting and leaving the inſtrument in that ſtate, when no receiver is upon it, the quickſilver in the gage will be riſing for about one hour, ſo as to aſcend one-tenth, or at moſt one-fifth of an inch above the ſurface of the quickſilver in the outward tube, and then it remains ſtationary; whereas if the experiment be repeated when the receiver is upon the plate, the degree of exhauſtion remains unaltered, the mercury not riſing at all in the ſmall tube. This riſing of the quickſilver in the firſt caſe ſeems to be occaſioned by ſome ſmall quantity of elaſtic fluid, that is yielded by the oil contained between the parts of the machine; for this quantity of elaſtic fluid can occaſion a ſenſible difference when the exhauſted

exhauſted ſpace within the pump is ſmall, but it becomes quite inconſiderable when the receiver is upon the plate, its quantity bearing a very ſmall proportion to the exhauſted ſpace.

It is of great advantage in this pump that very little oil can be lodged in it, becauſe then the elaſtic fluid yielded by this fluid is in ſo ſmall a quantity as not to affect the experiments. As for the oil, which by the working of the pump is accumulated into the oil veſſel, it cannot interfere with the exhauſtion of the pump, ſince it does not communicate with the cavity of the receiver.

The exhauſting power of this pump was next examined with the pear-gage, made and placed under a receiver after the uſual manner; and by this it appeared that the pump exhauſted ſo far as to remain within the receiver leſs than the thouſandth part of the air it contained before the exhauſtion *. In that caſe, the quickſilver in a ſhort barometer-gage came to the ſame level in both tubes, which proves that, by this laſt mentioned gage in that ſtate, the air is ſhewn to be rarefied at leaſt one thouſand times.

Laſtly, a long tube, or what is called a long barometer-gage, was adapted to the pump by means of a bent braſs tube. This glaſs tube went down along the ſide of the wooden frame, and its lower end was immerſed in ſome quickſilver kept in a proper ciſtern.

On working the pump, when all the three gages were annexed to it, there appeared, that the quickſilver of the ſhort gage came to the ſame level in the inſide as well as outſide tube,

* Whenever the pear-gage was uſed, care was taken to keep the inſide of the receiver, of the pear-gage, and of the pump, as free from moiſture as it was poſſible.

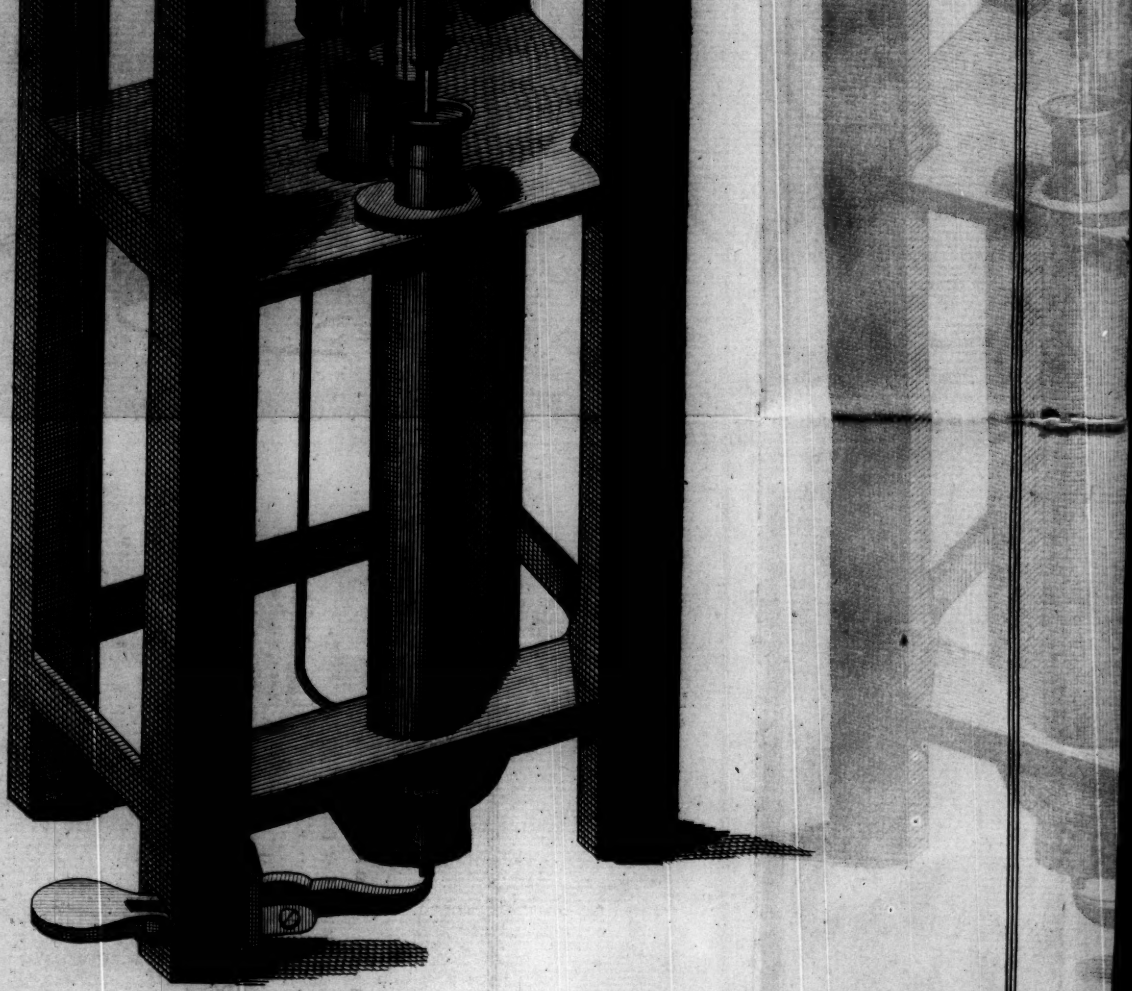
that the quickſilver in the long barometer-gage came as high as it ſtood at in a real barometer, and that the pear-gage indicated a degree of rarefaction about one thouſand. But as it was not known, whether the quickſilver had been boiled into the tube of the barometer, to which the long barometer-gage had been compared, therefore a barometer was accurately made for that purpoſe. The glaſs tube had been juſt drawn at the glaſs-houſe. It was perfectly clean; the quickſilver was boiled in the whole length of it, and care was taken that the dimenſions of the tube, ciſtern, and diviſions, were alike both in this barometer and in the gage. This done, the pump was tried again, and the quickſilver in the long gage roſe within one-twentieth of an inch of the quickſilver in the barometer, or rather leſs, which ſhews that the air was rarefied little above 600 times; but at this time the pump was not in proper order for trying ſuch nice experiments. It was leaky, and had not been taken to pieces and cleaned for above ſix weeks, in which time it had been frequently uſed, and continually left expoſed to the duſt of a working-ſhop: yet it ſhews, that in theſe unfavourable circumſtances this pump can rarefy the air above 600 times.

Conſidering all the above-mentioned circumſtances and experiments, I think it may be concluded, that this pump, when in good order, can rarefy the air about one thouſand times.

I ſhall, laſtly, conclude this paper with a ſummary account of ſeveral electrical experiments, which were made with this pump; reſerving to give a more ample and circumſtantial account of them for another opportunity.

When the air-pump was in good order, a glaſs receiver, which had a braſs cap cemented to its upper aperture or neck, was laid upon the plate; then the end of the prime conductor of an electric machine was placed within half an inch of the

cap



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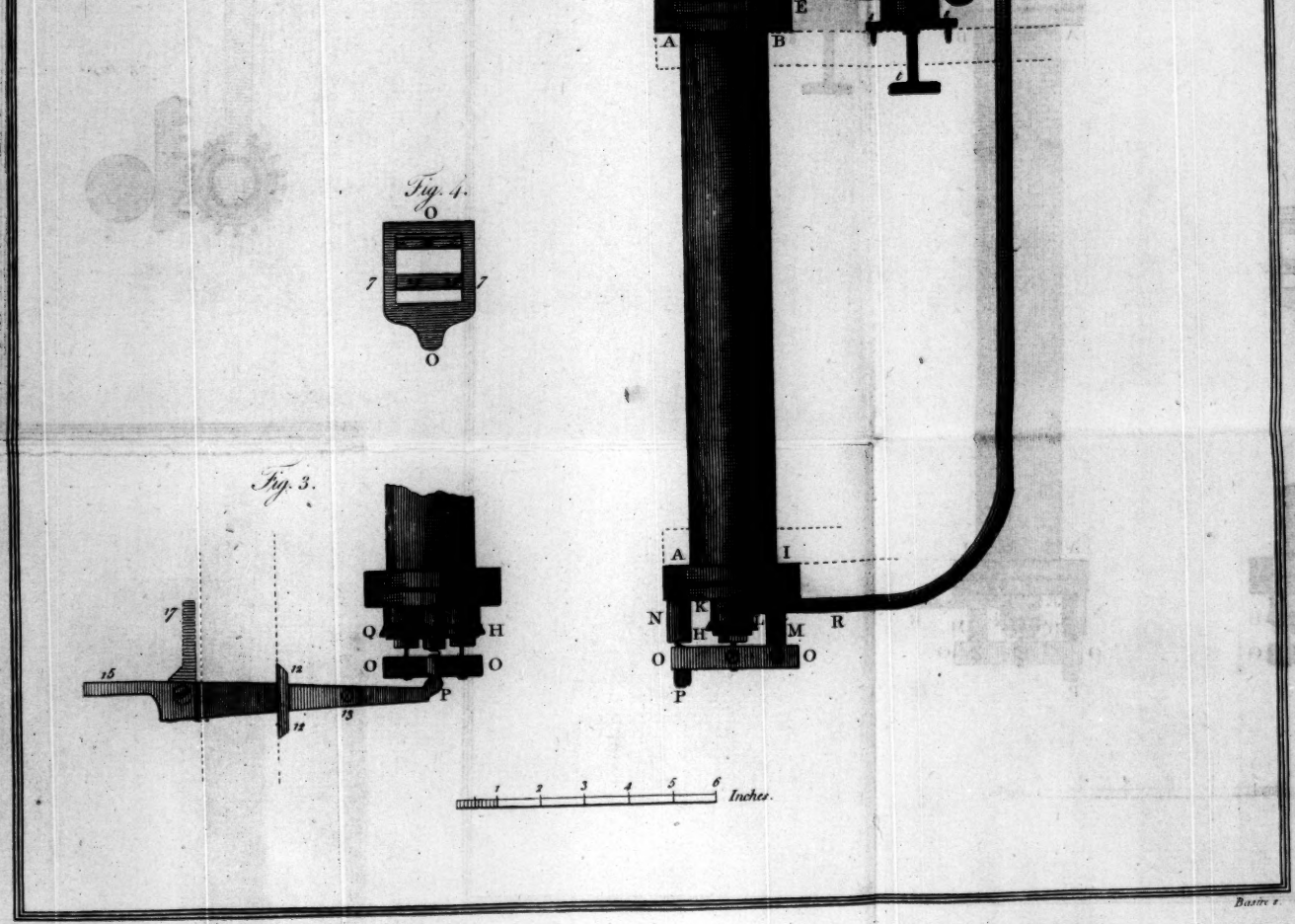

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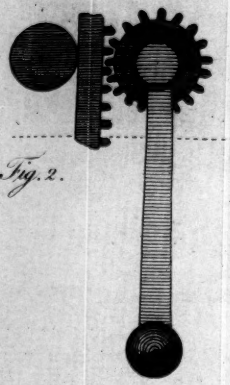

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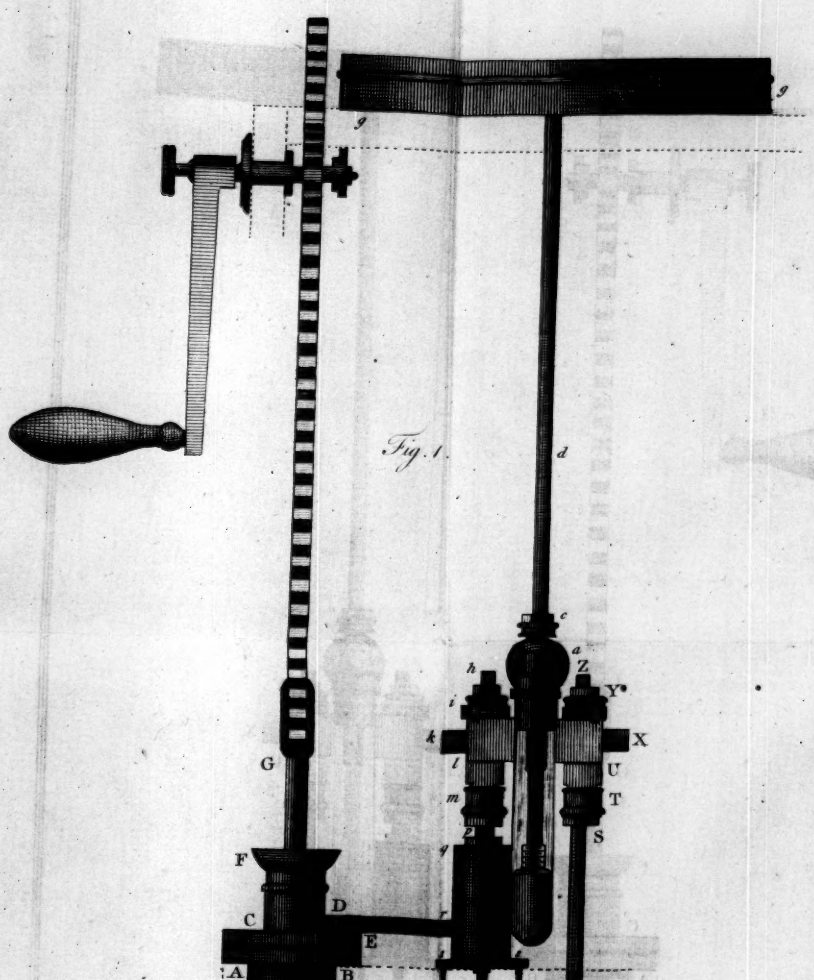
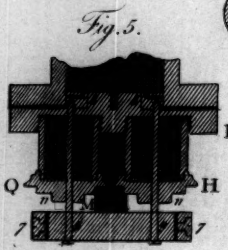
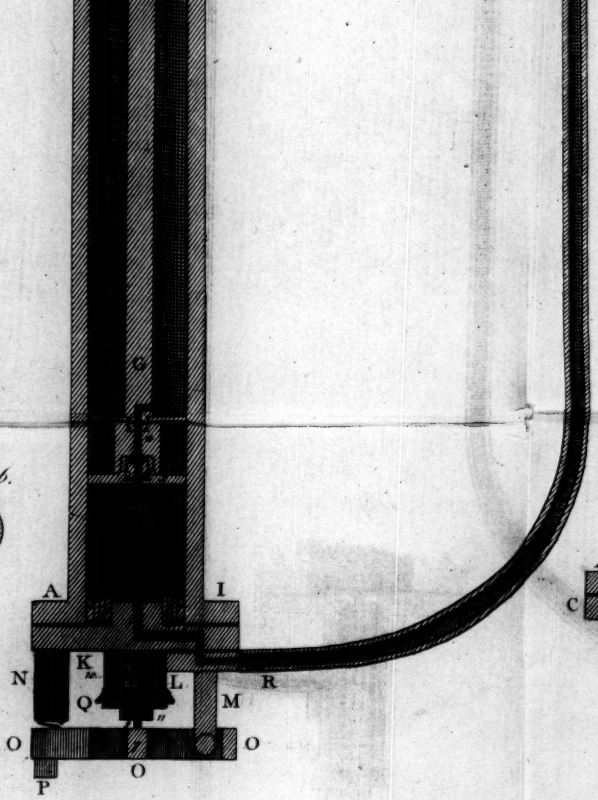
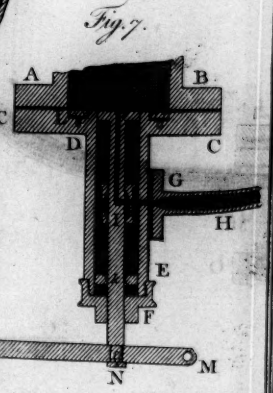


Fig. 1.







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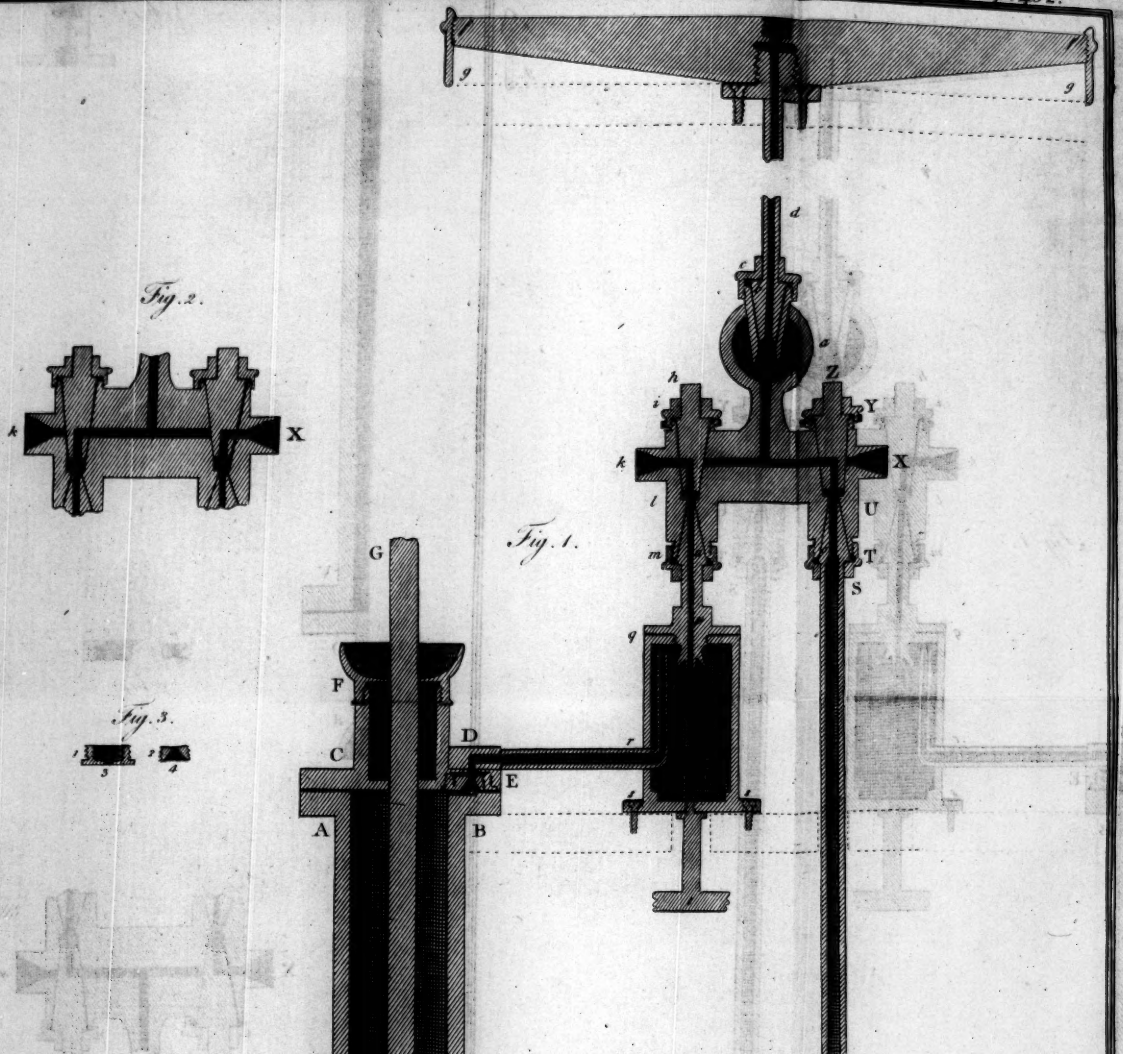
Fig. 2.
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cap of the receiver; ſo that when the machine was in action, the electric fluid in the form of ſparks went from the conductor to the braſs cap, and when the receiver was exhauſted, it paſſed from the cap to the plate of the pump through the receiver, illuminating its whole cavity. The more perfect the vacuum was made, the better conductor of electricity it became, and the electric light was more equably diffuſed in it; but it became by no means faint, even when the receiver was exhauſted to the utmoſt, though the light changed appearance according as the receiver was more or leſs exhauſted. Thoſe appearances were as follows.

Degrees of rarefaction as ſhewn by the ſhort gage.	Appearances of the electric light within the receiver.
Air rarefied 40 times.	Light in large, long, but divided, ſtreams.
70	Fine diffuſed light, of a white colour.
80	Beautiful diffuſed light inclining to red or purple, and filling the whole receiver.
100	
400	
When the gage ſhewed the utmoſt degree of exhauſtion.	A diffuſed light, filled equally every part of the cavity of the jar. It had hardly any reddiſh hue.

In this ſtate, or even when the air within the receiver was rarefied not above 100 times, if the braſs cap of the receiver was made to communicate with the ground by means of good conductors in perfect contact, the light within the receiver occaſioned by the electric ſparks given by the prime conductor to the cap of the receiver was thereby diminiſhed, but it did not

not intirely vaniſh; which ſhews, that the electric fluid, which proceeded from the conductor to the cap of the receiver in the form of ſparks, did not paſs to the earth all through the conductor, by which the cap was made to communicate with the ground; but part of it went at the ſame time through the vacuum, ſo that when the pump in this experiment was inſulated, ſparks could be drawn from the plate of it.

Having repeated the above mentioned experiments with only this variation, *viz.* that the extremity of the prime conductor was put in contact with the cap of the receiver, the appearances of electric light within the receiver were very nearly the ſame as in the preceding experiments; but if now the cap of the receiver was made to communicate with the ground, the light within the exhauſted receiver vaniſhed intirely, though the electrical machine acted very vigorouſly.

When a pith-ball electrometer was ſuſpended within the receiver from its braſs cap, and ſome electricity was communicated to the outſide of the ſaid cap, its balls diverged very little when the air within the receiver was rarefied about 100 times; their repulſion was hardly diſcernible when the air was rarefied about 300 times; but in a greater degree of rarefaction they did not diverge in the leaſt, and that was the caſe whether a ſmall or a large quantity of electric fluid was communicated to the cap of the receiver.

June 24, 1783.

XXIII. *Extract of a Letter from the Rev.* James Auguſtus Hamilton, *M. A. to the Rev.* Nevil Maſkelyne, *D. D. F. R. S. giving an Account of his Obſervation of the Tranſit of Mercury over the Sun, of Nov.* 12, 1782, *obſerved at* Cook's-Town, *near* Dungannon, *in Ireland.*

Read April 20, 1783.

Cook's-Town in Ireland, Nov. 16, 1782.

SIR,

FROM the very indulgent eye with which you regarded my aſtronomical wiſhes, when I had the pleaſure of ſeeing you at Greenwich, I make no doubt of your pardon for offering my imperfect efforts towards obſerving the late tranſit of Mercury. I do not mean to trouble you with the peruſal of the labours of the week, but only to ſtate to you my general apparatus and reſults, and to requeſt your kind communication of your obſervation of the ſame phænomenon, if it occurred at Greenwich. I have only a good common clock with a varniſhed deal pendulum, moving ſidereal time; a tranſit-inſtrument, of four feet eight inches focal diſtance, with a triple object-glaſs, and the axis of two feet, on ſolid ſtone pillars, the baſe common to both, each pillar being a ſingle ſtone of five feet high: the collimation is conſtantly attended to, and was examined a day or two preceding the tranſit by an obſervation of the * polar ſtar (the

* The weather being hazy prevented the horizontal meridian mark being diſtinctly obſervable.

only circumpolar one my ſituation enables me to ſee an intire revolution of) and I found on inverting the axis that the error of collimation amounted to only 2″ of time at the polar ſtar. By an obſervation of the ſun's paſſage before the tranſit, the clock was ſlow − 21″,7; and by an obſervation of the paſſage of Lyra corrected ſtrictly by your tables, after the ingreſs of Mercury, the clock appeared to be 21″,4 ſlow, its rate for the week loſing 1″,5 *per* revolution. I obſerved with an achromatic tube of three inches aperture, triple object-glaſs, and uſed a magnifying power of about 90 times, which I preferred on account of the ſtate of the atmoſphere. At about two o'clock I ſet a ſtop watch to apparent ſolar time, and placed myſelf at the teleſcope within hearing of the beat of the tranſit-clock. I kept the part of the diſk where I expected the ingreſs in conſtant view, my ſight being directed by a vertical wire in the eye-tube, and at 2 h. 22′ 3″ I ſtopped the watch, and counted 20″, to be ſure of my having really perceived the firſt impreſſion (which I apprehend could not have been ſhewn 1″ ſooner by the power, &c. I uſed). I then ſtopped ſeconds to the clock, and counted up to an even minute, and found, that the firſt external contact happened at 17 h. 33′ 11″ by the clock, or 2 h. 21′ 45″ apparent time. Mercury came in like a diſtinct black point, without any preceding hazineſs or appearance of atmoſphere; and at 17 h. 39′ 10″ by the clock, or 2 h. 27′ 43″ apparent time, the thread of light ſeemed compleated, and then I date the internal contact. I had no inſtrument fit to take any micrometer meaſures, ſo continued only looking at the planet till the ſun got ſo low, that the limb preſented the appearance of a troubled ſea at a diſtant horizon, among the waves of which Mercury once more plunged at about 18 h. 52′, and the ſun and

planet both left my view at about 18 h. 57′; but theſe obſervations are only good conjectures. From my beſt obſervations of eclipſes of Jupiter's firſt ſatellite, of appulſes of the moon's centre to the meridians, and lunar diſtances with a HADLEY's quadrant, I make my longitude 26′ 35″ W. (nearly), and my latitude by a mean of many obſervations, is 54° 38′ 20″.

I have the honour to be, &c.

XXV. *Methodus Inveniendi Lineas Curvas ex proprietatibus Variationis Curvaturæ. Auctore* Nicolao Landerbeck, *Mathes. Profess. in Acad. Upsalienſi Adjuncto: communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read March 13, 1783.

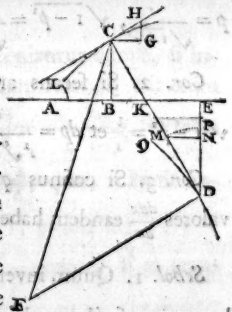
PARS PRIMA.

QUALITAS curvaturæ in diverſis lineis diverſiſque earum punctis diverſa reperitur. Circulo ubique eadem eſt curvatura, quæ in alia quavis curva, continue creſcendo vel decreſcendo, figuram ab uniformi circuli variat; quo enim majori velocitate progrediens creſcit vel decreſcit curvaturæ radius, eo citius curvæ a circuli oſculatorii curvatura deflectit; et quo majori celeritate iſochrona ipſa curva creſcit vel decreſcit, eo citius fertur motu angulari radius curvedinis et remotius idem curvaturæ gradus locum obtinet, quo circulus curvam oſculans eam in angulo majori vel minori in puncto contactus ſimul ſecat. Hæc curvaturæ a circulari aberratio, quæ curvaturæ variatio nuncupatur, etſi alia in alia curva gaudeat proprietate, menſurari et exprimi poteſt generaliter per rationem fluxionum radii curvedinis et curvæ, quæ ratio proinde variationis index cenſenda eſt, ut in opere, quod Methodus Fluxionum inſcribitur, illuſtriſſimus NEWTONUS nos docuit. Demonſtravit præterea MACLAURINUS in propoſitione trigeſima ſexta Tractatus de Fluxionibus, quod index hic variationis curvaturæ curvæ cujuſcunque ſit ut tangens anguli, linea punctum in curva et centrum curvaturæ evolutæ jungente et radio curvaturæ in iſto puncto comprehenſi; cujus analytica expreſſione, quæ pro quavis curva calculo differentiali facile habetur, intima curva-

rum examinare licet, ut non ſolum punctum ejuſdem curvæ, ubi inequabilitas curvaturæ eſt vel nulla vel datæ magnitudinis vel minima vel maxima vel infinita determinare, ſed etiam curvas inter ſe comparare valeant matheſeos periti, ut quibus punctis curvatura ſit æqualis et ſimilis diſcernere queant. Methodum ex proprietatibus variationis curvaturæ inveniendi curvas explicatam adhuc non vidi, quæ, ſi detecta et explicata fuerit, quantum matheſeos ſcientiæ interſit, quemque præbeat uſum in problematibus tam mathematicis quam phyſicis ſolvendis, quæ a curvatura dependent, mathematicorum eſt judicare, quorum etiam judicio, quæ ad methodum hanc explicandam feci tentamina ſubjicio.

THEOREMA I.

Si curvæ cujuſdam LC, ad axin concavæ vel convexæ, index variationis curvaturæ, ſeu tangens anguli DCF, radio curvaturæ CD in puncto C et linea CF, punctum C et centrum curvaturæ F evolutæ QD jungente, comprehenſi, dicatur T, ſinus anguli BCD p, poſito ſinu toto 1, arcus curvæ LC z, coordinatæ orthogonales AB, BC x et y earumque fluxiones dp, dz, dx et dy reſpective dicantur, erit $\frac{ddx}{dx} = -\frac{T\,dp}{\sqrt{1-p^2}}$.



Sumatur DM unitati æqualis et ducantur DE axi AB et MN ipſi DE normales, et deſcribatur arcus circuli MP; erit $MN = p$ et $DN = \sqrt{1-p^2}$. Quoniam ob ſimilitudinem triangulorum DNM et CHG, erit $DN\,(\sqrt{1-p^2}) : MN\,(p) :: CG\,(dx) : GH$

(dy) et $dy = \frac{pdx}{\sqrt{1-p^2}}$, eamque ob cauſſam DN $(\sqrt{1-p^2})$: DM (1) :: CG (dx) : CH (dz) et $dz = \frac{dx}{\sqrt{1-p^2}}$. Si radius curvaturæ CD ſit R et ponatur conſtans, ejus enim fluxio ex coordinatarum non dependet, erit lineæ BE fluxio $= -dx$. Propter ſimilitudinem triangulorum CBK, KED et NDM erit DM (1) : MN (p) :: CK + KD (R) : BE $= Rp$, cujus fluxio $R\,dp = -dx$ et $R = -\frac{dx}{dp}$, et ſi hujus æquationis fluxiones ſumantur, poſita dp conſtante, habetur $dR = -\frac{ddx}{dp}$, quæ per $dz = \frac{dx}{\sqrt{1-p^2}}$ diviſa dat $T\left(=\frac{dR}{dz}\right) = -\frac{ddx\sqrt{1-p^2}}{dx\,dp}$, qua prodit $\frac{ddx}{dx} = -\frac{T\,dp}{\sqrt{1-p^2}}$.

Cor. 1. Si tangens anguli BCD deſignetur per r, erit $p = \frac{r}{\sqrt{1+r^2}}$, $\sqrt{1-p^2} = \frac{1}{\sqrt{1+r^2}}$ et $dp = \frac{dr}{1+r^2}$, unde $\frac{ddx}{dx} = -\frac{T\,dr}{1+r^2}$.

Cor. 2. Si ſecans anguli BCD dicatur S, erit $p = \frac{\sqrt{s^2-1}}{s}$, $\sqrt{1-p^2} = \frac{1}{s}$ et $dp = \frac{ds}{s^2\sqrt{s^2-1}}$, quo $\frac{ddx}{dx} = -\frac{T\,ds}{s\sqrt{s^2-1}}$.

Cor. 3. Si coſinus q, cotangens t et coſecans v dicantur, valores $\frac{ddx}{dx}$ eandem habent formam, ſignis mutatis.

Schol. 1. Quum inventa ſit $T = -\frac{ddx\sqrt{1-p^2}}{dx\,dp}$, methodum habemus perfacilem calculandi generaliter variationem curvaturæ uniuſcujuſque curvæ; data enim relatione inter fluxiones coordinatarum, quæ per æquationem hujus formæ $dy = X\,dx$ exhibetur, ubi X functio eſt abſciſſæ x, datur $\frac{p}{\sqrt{1-p^2}} = X$, qua x per p et p per x exprimi poteſt. Si variatio curvaturæ per p expreſſa deſideretur, ponatur $x = P$, quantitatis p functioni, et fluxioni-

bus

bus primis $dx = \dot{P}dp$ et ſecundis $ddx = \ddot{P}dp^2$, poſita dp conſtante, ſumtis, valoribuſque pro dx et ddx ſubſtitutis, habetur curvæ propoſitæ index variationis curvaturæ $T = -\frac{P\sqrt{1-p^2}}{\dot{P}}$, denotantibus $\ddot{P}$ et $\dot{P}$ functiones quantitatis p. Si vero index variationis curvaturæ exprimenda ſit per x, æquatione $X = \frac{p}{\sqrt{1-p^2}}$ inveniatur $p = \dot{X}$ et $\sqrt{1-p^2} = \sqrt{1-\dot{X}^2}$, ſumtiſque æquationis $p = \dot{X}$ primis et ſecundis fluxionibus, dp conſtante habita, erit $dp = \ddot{X}dx$ et $o = \ddot{X}ddx + \dddot{X}dx^2$, qua $ddx = -\frac{\dddot{X}dx^2}{\ddot{X}}$, et ſubſtitutione debita $T = \frac{\ddot{X}\sqrt{1-X^2}}{\dot{X}}$, ſignificantibus $\dddot{X}$, $\ddot{X}$, et $\dot{X}$ functiones abſciſſæ x.

Schol. 2. Hoc adhibito theoremate inveniantur curvæ, ſi inter T et p, T et r vel T et s detur quædam relatio. Sit enim $T = P$, functioni quantitatis p, habetur $\frac{ddx}{dx} = -\frac{Pdp}{\sqrt{1-p^2}}$, et facta integratione $\log. dx = -\int\frac{Pdp}{\sqrt{1-p^2}} + \log. Adp$, quæ, ſi N ſit numerus, cujus logarithmus hyperbolicus 1, evadit $\log. dx = -\log. N\int\frac{Pdp}{\sqrt{1-p^2}} + \log. Adp$, et ſi $N\int\frac{Pdp}{\sqrt{1-p^2}}$ ponatur F et tranſeundo a logarithmis ad quantitates abſolutas, erit $dx = \frac{Adp}{F}$, cujus ſi ſumantur integralia, obtinetur $x + C = \int\frac{Adp}{F}$, qua equatione p per x exprimi poſſit. Sit $p = X$, functioni abſciſſæ x, erit $\sqrt{1-p^2} = \sqrt{1-X^2}$, $dy\left(=\frac{pdx}{\sqrt{1-p^2}}\right) = \frac{Xdx}{\sqrt{1-X^2}}$ et integratione $y = \int\frac{Xdx}{\sqrt{1-X^2}}$ æquatio, qua curvarum natura innoteſcit.

Patet

Patet hinc, quod, quoties $\int \frac{P dp}{\sqrt{1-p^2}}$ per logarithmos ſumi non poſſit, curva, quæ quæritur, ſit tranſcendens; ut vero ſit algebraica, requiritur, non ſolum ut $\int \frac{P dp}{\sqrt{1-p^2}}$ ſit integrale logarithmicum, ſed etiam ut $\int \frac{A dp}{F}$ et $\int \frac{X dx}{\sqrt{1-X}}$ ſint quantitates, quæ abſolutam admittant æquationem.

Exempl. 1. Si invenienda ſit curva, cujus variatio curvaturæ $T = \frac{3\sqrt{1-p^2}}{p}$. Per theorema habetur $\frac{ddx}{dx}\left(= -\frac{T dp}{\sqrt{1-p^2}}\right) = -\frac{3dp}{p}$, quam æquationem integrando et corrigendo prodit log. dx $\left(= \log. \frac{1}{p^3} + \log. -\frac{adp}{2}\right) = \log. -\frac{adp}{2p^3}$, et a logarithmis ad quantitates abſolutas tranſeundo $dx = -\frac{adp}{2p^3}$, et iterum integrando et corrigendo $x + C\left(= -\int \frac{a dp}{2p^3}\right) = \frac{a}{4p^2}$, ex qua æquatione habetur $p = \frac{\sqrt{a}}{2\sqrt{C+x}}$ et $\sqrt{1-p^2} = \frac{\sqrt{4C+4x-a}}{2\sqrt{C+x}}$, unde ſequitur, quod ſit $y\left(= \int \frac{p dx}{\sqrt{1-p}}\right) = \int \frac{\sqrt{a} \cdot dx}{\sqrt{4C+4x-a}}\right) = \sqrt{a} \cdot \sqrt{4C+4x-a}$, qua æquatione conſtat, curvam eſſe parabolam apollonianam, cujus parameter principalis a.

Exempl. 2. Si curva quæritur, cujus variatio curvaturæ $T = \frac{1-3p^2}{p\sqrt{1-p^2}}$, theoremate habetur $\frac{ddx}{dx}\left(= -\frac{T dp}{\sqrt{1-p^2}}\right) = \frac{3p^2-1 \cdot dp}{p \cdot 1-p^2}$, cujus æquatio integralis correcta erit log. dx $\left(= \log. \frac{1}{p \cdot 1-p^2} + \log. adp\right) = \log. \frac{adp}{p \cdot 1-p^2}$, vel, facto a logarithmis tranſitu, $\frac{dx}{a} = \frac{dp}{p \cdot 1-p^2}$ et integratione $\frac{x}{a} + C = \log. \frac{p}{\sqrt{1-p^2}}$, unde ſi N ſit numerus,

merus, cujus logarithmus hyperbolicus 1, erit $\frac{p}{\sqrt{1-p^2}} = N^{\frac{x}{a}} + C$ et $y \left(= \int \frac{p\,dx}{\sqrt{1-p^2}}\right) = \int N^{\frac{x}{a}} + C\,dx$, curva igitur eſt logarithmica.

Exempl. 3. Si curvaturæ variatio ſit $T = \frac{3 \cdot \sqrt{a^2 \mp b^2} \cdot r}{a^2 r^2 \pm b^2}$, quæritur curva. Per corollarium primum habetur $\frac{dx}{dx} \left(= -\frac{T dr}{1+r^2}\right) = -\frac{3 \cdot \sqrt{a^2 \mp b^2} \cdot r\,dr}{a^2 r^2 \pm b^2 \cdot 1+r^2}$ et integratione facta $\log. dx \left(= \log. \frac{1+r^2}{a^2 r^2 \pm b^2}^{\frac{3}{2}} + \log. \pm \frac{b^2 a^2 dr}{2 \cdot 1+r^2|^{\frac{3}{2}}}\right) = \log. \pm \frac{b^2 a^2 dr}{2 \cdot a r \pm b^2|^{\frac{3}{2}}}$, vel, ſumendo quantitates abſolutas, $\mp dx = \frac{b^2 a^2 dr}{2 \cdot a^2 r^2 \pm b^2|^{\frac{3}{2}}}$, et integratione $C \mp x = \frac{a^2 r}{2\sqrt{a^2 r^2 \pm b^2}}$, ex qua æquatione $r = \frac{b \cdot \sqrt{2C \mp 2x}}{a\sqrt{2C \mp 2x|^2 - a^2}}$ et $y \left(= \int r dx\right) = \int \frac{b \cdot \sqrt{2C \pm 2x} \cdot dx}{a\sqrt{2C \mp 2x|^2 - a^2}}$, æquatio indolem curvarum exprimens, quæ ſi $C = \frac{a}{2}$ erit $y = \frac{b\sqrt{ax \mp x^2}}{a}$, æquatio pro ſectionibus conicis.

Exempl. 4. Proponatur invenire curvam, cujus curvaturæ variatio $T = \frac{2 \cdot 2p^2 - 3}{p^2 - 2 \cdot \sqrt{p^2 - 1}}$, per ſecantem anguli BCD expreſſa, datur. Per corollarium ſecundum curvam conſequi licet; ſed per ſubſtitutionem $T = \frac{2 \cdot 3p^2 - 1\sqrt{1-p^2}}{p \cdot 2p^2 - 1}$ habetur, erit $\frac{dds}{dx} \left(= -\frac{T dp}{\sqrt{1-p^2}}\right) = \frac{2 \cdot \sqrt{1-3p^2} \cdot dp}{p \cdot 2p^2 - 1}$, integratione $\log. dx \left(= \log. \frac{1}{p^2\sqrt{2p^2-1}} + \log. adp\right) = \log. \frac{adp}{p^2\sqrt{2p^2-1}}$ et adhibendo quantitates abſolutas

ſolutas $dx = \frac{a^2 p}{p^2\sqrt{2p^2-1}}$ cujus æquatio integralis $x + C = \frac{a\sqrt{2p^2-1}}{p}$ dat $p = \frac{a}{\sqrt{2a^2-x+C^2}}$ et $\sqrt{1-p^2} = \frac{\sqrt{a^2-x+C^2}}{\sqrt{2a^2-x+C^2}}$, quo $y\left(=\int\frac{p\,dx}{\sqrt{1-p^2}}\right)$ $=\int\frac{a\,dx}{\sqrt{a^2-x+C^2}}$ æquatio pro curva, quæ ſinuum vocatur.

THEOREMA II.

Si coſinus anguli BCD ſit q, poſito radio 1, et reliquæ determinationes maneant ut in theoremate præcedenti, erit $\frac{ddy}{dy} = \frac{T\,dq}{\sqrt{1-q^2}}$.

Nam propter triangulorum DMN et CHG ſimilitudinem MN $(\sqrt{1-q^2})$: DN (q) :: HG (dy) : CG (dx) et MN $(\sqrt{1-q^2})$: MD (1) :: HG (dy) : CH (dz) erit $dx = \frac{q\,dy}{\sqrt{1-q^2}}$ et $dz = \frac{dy}{\sqrt{1-q^2}}$. Per ſimilitudinem triangulorum CDK, KED, et NDM, erit MD (1) : DN (q) :: DK + KC (R) : y + DE, unde $Rq = y$ + DE, ſumptiſque fluxionibus $R\,dq = dy$, qua $R = \frac{dy}{dq}$, radius enim curvaturæ ut conſtans ſuppoſitus, DE etiam conſtans erit, et ſi ulterius ſumantur fluxiones, dq conſtante habita, erit $dR = \frac{ddy}{dq}$, qua diviſa per $dz = \frac{dy}{\sqrt{1-q^2}}$ provenit T $\left(=\frac{dR}{dz}\right) = \frac{ddy\sqrt{1-q^2}}{dy\,dq}$ et $\frac{ddy}{dy} = \frac{T\,dq}{\sqrt{1-q^2}}$.

Cor. 1. Si cotangens anguli BCD dicatur t, erit $q = \frac{t}{\sqrt{1+t^2}}$, $\sqrt{1-q^2} = \frac{1}{\sqrt{1+t^2}}$, $dq = \frac{dt}{1+t^2}$ et $\frac{ddy}{dy} = \frac{T\,dt}{1+t^2}$.

Cor.

Cor. 2. Si cosecans anguli BCD fit v, erit $q = \frac{\sqrt{v^2-1}}{v}$, $\sqrt{1-q^2} = \frac{1}{v}$, $dq = \frac{dv}{v^2\sqrt{v^2-1}}$ et $\frac{ddy}{dy} = \frac{T dv}{v\sqrt{v^2-1}}$.

Schol. 1. Si per æquationem hujus formæ $dx = Y dy$, ubi Y functio est ordinatæ y, relatio datur inter coordinatarum fluxiones æquatione $T = \frac{ddy\sqrt{1-q^2}}{dyaq}$, eodem calculandi modo ac in scholio 1. variatio curvaturæ $T = \frac{\overset{\text{II}}{Q}\sqrt{1-q^2}}{\overset{\text{I}}{Q}}$ generaliter in q habetur, significantibus $\overset{\text{II}}{Q}$ et $\overset{\text{I}}{Q}$ functiones cosinus q. Pari calculandi ratione ac in eodem Scholio curvaturæ variatio $T = -\frac{\overset{\text{III}}{Y}\sqrt{1-\overset{\text{II}}{Y}^2}}{\overset{\text{II}}{Y}}$, denotantibus $\overset{\text{III}}{Y}$, $\overset{\text{II}}{Y}$ et $\overset{\text{I}}{Y}$ functiones ordinatæ y, inveniri potest.

Schol. 2. Per hoc theorema natura curvæ habetur ex data relatione inter T et q, T et r vel T et s, &c. Nam si sit $T = Q$, functioni cosinus q, erit $\frac{ddy}{dy} = \frac{Q dq}{\sqrt{1-q^2}}$, et integratione log. $dy = \int \frac{Q dq}{\sqrt{1-q^2}} + \log. B dq$, vel log. $dy = \log. N \int \frac{Q dq}{\sqrt{1-q^2}} + \log. B dq$, si N sit numerus, cujus logarithmus hyperbolicus 1; et si $N \int \frac{Q dq}{\sqrt{1-q^2}}$ dicatur G, et facto a logarithmis transitu, prodit $dy = \frac{B dq}{G}$, et per integrationem $y + C = \int \frac{B dq}{G}$ ex qua q in y datur. Sit $q = Y$, functioni ordinatæ y, erit $\sqrt{1-q^2} = \sqrt{1-Y^2}$ et $x \left(= \int \frac{q dy}{\sqrt{1-q^2}}\right) = \int \frac{Y dy}{\sqrt{1-Y^2}}$ generalis æquatio, indolem curvarum exprimens.

Ad hæc idem eſt obſervandum ac in theoremate præcedenti, quod ſi $\int \frac{Q\,dq}{\sqrt{1-q^2}}$ integrale ſit logarithmicum et $\int \frac{B\,dp}{G}$ et $\int \frac{Y\,dy}{\sqrt{1-Y^2}}$ quantitates perfecte integrabiles, curva evadit algebraica, ſi vero aliter evenerit, ſemper tranſcendens.

Ex. 1. Propoſitum eſto invenire curvam, cujus variatio curvaturæ $T = \frac{1}{q\sqrt{1-q^2}}$. Per theorema habetur $\frac{ddy}{dy}\left(= \frac{T\,dq}{\sqrt{1-q^2}}\right) = \frac{dq}{q\,.\,1-q^2}$, integratione et correctione peracta, log. dy $\left(= \log. \frac{q}{\sqrt{1-q^2}} + \log. - a\,dq\right) = \log. - \frac{aq\,dq}{\sqrt{1-q^2}}$, et adhibendo quantitates abſolutas $dy = -\frac{aq\,dq}{\sqrt{1-q^2}}$, et denuo integrando erit $y + C\left(= -a\int \frac{q\,dq}{\sqrt{1-q^2}}\right) = a\sqrt{1-q^2}$, unde $\sqrt{1-q^2} = \frac{Y+C}{a}$ et $q = \frac{\sqrt{a^2 - Y + C|^2}}{a}$ et $x\left(= \int \frac{q\,dy}{\sqrt{1-q^2}}\right) = \frac{dy\sqrt{a^2 - y + C|^2}}{y+C}$ et ſi $C = o$ pro venit $x = \int \frac{dy\sqrt{a^2-y^2}}{y}$, qua conſtat, curvam eſſe tractoriam.

Ex. 2. Quænam eſt curva, cujus curvaturæ variatio $T = \frac{3q^2 - 2}{q\sqrt{1-q^2}}$? Vi theorematis habetur $\frac{ddy}{dy}\left(= \frac{T\,dq}{\sqrt{1-q^2}}\right) = \frac{3q^2 - 2\,dq}{q\,.\,1-q^2}$, integratione et correctione $\log. dy\left(= \log. \frac{1}{q\sqrt{1-q^2}} + \log. - a\,dq\right) = \log. - \frac{a\,dq}{q\sqrt{1-q^2}}$, hoc eſt $dy = -\frac{a\,dq}{q\sqrt{1-q^2}}$, etiterum integrando $y + C\left(= -a\int \frac{dq}{q\sqrt{1-q^2}}\right) = \frac{a\sqrt{1-q^2}}{q}$, qua habetur $\frac{q}{\sqrt{1-q^2}} = \frac{a}{y+C}$ et $x\left(= \int \frac{q\,dy}{\sqrt{1-q^2}}\right) = \int \frac{a\,dy}{y+C}$, et ſi $C = o$, $x = \int \frac{dy}{y}$ æquatio pro logarithmica ordinaria.

THEOREM

THEOREMA III.

Manentibus iisdem ac in theoremate primo, erit $\frac{ddz}{dz} = -\frac{T dp}{\sqrt{1-p^2}}$ vel etiam $\frac{ddz}{dz} = \frac{T dq}{\sqrt{1-q^2}}$.

Est enim $dz = \frac{dx}{\sqrt{1-p^2}}$ et $dx = dz\sqrt{1-p^2}$, quare $R\left(= -\frac{dx}{dp}\right) = -\frac{dz\sqrt{1-p^2}}{dp}$, cujus fluxiones $dR = -\frac{ddz\sqrt{1-p^2}}{-dp}$, posita arcus MP fluxione $\frac{dp}{\sqrt{1-p^2}}$ constante, per dz divisæ dant $T\left(= \frac{dR}{dz}\right) = -\frac{ddz\sqrt{1-p^2}}{dz\,dp}$, qua sequitur $\frac{ddz}{dz} = -\frac{T dp}{\sqrt{1-p^2}}$. Et quum fluxio arcus circuli æqualis sit negativæ fluxioni complimenti, erit etiam $\frac{ddz}{dz} = \frac{T dq}{\sqrt{1-q^2}}$.

Cor. Si sint ut antea tangens anguli BCD, r et secans s, habetur $\frac{ddz}{dz} = -\frac{dr}{1+r^2} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Si alterutra æquationum formæ $dx = Zdz$ et $dy = Zdz$, inter fluxiones abscissæ vel ordinatæ et curvæ, relatio detur, per formulam $T = -\frac{ddz\sqrt{1-p^2}}{dz\,dp}$ vel $T = \frac{ddz\sqrt{1-q^2}}{dz\,dq}$, variatio curvaturæ in p, $-\frac{P\sqrt{1-p^2}}{P}$, in q $\frac{Q\sqrt{1-q^2}}{Q}$, et in z $\frac{Z\sqrt{1-Z^2}}{Z}$, eodem ac antea habetur, posita fluxione quantitatis $\int\frac{dp}{\sqrt{1-p^2}}$ constante.

Schol. 2. Ope hujus theorematis invenire licet indolem curvæ, si inter T et p, T et q, &c. relatio detur. Sit $T = P$, functioni

sinus p, erit $\frac{ddz}{dz} = -\frac{Pdp}{\sqrt{1-p^2}}$, facta integratione et correctione debita, $\log. dz = -\int\frac{Pdp}{\sqrt{1-p^2}} + \log.\frac{Edp}{\sqrt{1-p^2}}$, vel $\log. dz = -\log. N\int\frac{Pdp}{\sqrt{1-p^2}} + \log.\frac{Edp}{\sqrt{1-p^2}}$, si N sit basis logarithmorum hyperbolicorum, atque posita $N\int\frac{Pdp}{\sqrt{1-p^2}} = H$, et facto de logarithmis transitu, $dz = \frac{Edp}{H\sqrt{1-p^2}}$, et iterum integrando $z + C = \int\frac{Edp}{H\sqrt{1-p^2}}$, unde p per z habetur. Sit $p = Z$, functioni arcus curvæ z, erit $\sqrt{1-p^2} = \sqrt{1-Z^2}$, $x\ (=\int dz\sqrt{1-p^2}) = \int dz\sqrt{1-z^2}$ et $y\ (=\int pdz) = \int Zdz$, quorum alterutra curvarum indoles cognoscitur. Pari modo procedendum est, si $T = Q$, quantitatas q functioni.

Hinc facile colligitur, quod, quoties $\int\frac{Pdp}{\sqrt{1-p^2}}$ sit integrale logarithmicum et quantitates $\int\frac{Edp}{H\sqrt{1-p^2}}$ et $\int dz\sqrt{1-Z^2}$ vel $\int Zdz$ perfectæ integrabiles, curvæ erunt rectificabiles et algebraicæ, quoties relatio inter x et z vel inter y et z in relationem algebraicam x et y resolvi possit.

Exempl. 1. Si desideretur curva, cujus curvaturæ variatio $T = \frac{2\sqrt{1-p^2}}{p}$. Per theorema est $\frac{ddz}{dz}\left(= -\frac{Tdp}{\sqrt{1-p^2}}\right) = -\frac{2dp}{p}$ et integratione $\log. dz\ \left(= \log.\frac{1}{p^2} + \log.\frac{adp}{\sqrt{1-p^2}}\right) = \log.\frac{adp}{p^2\sqrt{1-p^2}}$, qua $dz = \frac{adp}{p^2\sqrt{1-p^2}}$, et denuo integrando $z + C = -\frac{a\sqrt{1-p^2}}{p}$, qua habetur

betur $p = \frac{a}{\sqrt{a^2+z+C|^2}}$, $\sqrt{1-p^2} = \frac{z+C}{\sqrt{a^2+z+C|^2}}$ et x $(= \int dz\sqrt{1-p^2}) = \frac{\overline{z+C}\cdot dz}{\sqrt{a^2+z+C|^2}}$; si $C=0$, evadit x $(= \int \frac{z\,dz}{\sqrt{a^2-z^2}})$ $= -a+\sqrt{a^2-z^2}$, curva igitur est catenaria.

Exempl. 2. Sit variatio curvaturæ $T = \frac{\sqrt{1-q^2}}{q}$, quæritur curva. Vi theorematis erit $\frac{ddz}{dz}\left(= \frac{T\,dq}{\sqrt{1-q^2}}\right) = \frac{dq}{q}$ et integratione log. dz $(=$ log. $q +$ log. $\frac{a\,dq}{\sqrt{1-q^2}})$ $=$ log. $\frac{a\,q\,dq}{\sqrt{1-q^2}}$, qua $dz = \frac{a\,q\,dq}{\sqrt{1-q^2}}$ et rursus integrando $z+C = -a\sqrt{1-q^2}$, unde $q = \frac{\sqrt{a^2-z+C|^2}}{a}$, $\sqrt{1-q^2} = \frac{z+C}{a}$ et y $(= \int dz\sqrt{1-q^2}) = \int \frac{\overline{z+C}\cdot dz}{a}$, si $C = -a$ patet curvam esse cycloidem.

THEOREMA IV.

Retentis antea adhibitis denominationibus, erit $\frac{dR}{RT} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam DM (1) : CD (R) :: $-\frac{dp}{\sqrt{1-p^2}} : dz$ habetur $dz = -\frac{R\,dp}{\sqrt{1-p^2}}$, quæ æquatio per T multiplicata dat $T\,dz = -\frac{RT\,dp}{\sqrt{1-p^2}}$, et quum $dR = T\,dz$, prodit $\frac{dR}{RT} = -\frac{dp}{\sqrt{1-p^2}}$.

Schol. 1. Hujus theorematis subsidio inveniri potest curvarum indoles, si inter R et T detur quædam relatio. Sit $R = K$, quantitatis T functioni, habetur per hoc theorema $\frac{dK}{KT} = -\frac{dp}{\sqrt{1-p^2}}$,

et

et facta integratione $\int \frac{dK}{KT} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Quoniam $-\int \frac{dp}{\sqrt{1-p^2}}$ arcus est circuli, cujus sinus $\sqrt{1-p^2}$, si ponatur $\int \frac{dK}{KT} + C = \eta$ et N numerus, cujus logarithmus hyperbolicus 1, erit $\sqrt{1-p^2} = \frac{N^{\eta\sqrt{-1}} - N^{-\eta\sqrt{-1}}}{2\sqrt{-1}}$, functioni quantitatis T, unde per hanc æquationem T in p vel substitutione T in q vel r, &c. exprimi potest. Cognita relatione inter T et p vel T et q, r, &c. relationem inter coordinatas vel inter curvam et abscissam vel ordinatam per theoremata præcedentia inveniendi aditus patet.

Hinc facile colligitur, quod quoties $\int \frac{dK}{KT}$ non sit per arcus circulares integrabilis curva semper sit trrnscendens.

Ex. 1. Quænam est curva, si relatio inter R et T per æquationem $R = \frac{a \cdot \sqrt{4+T^2}}{4}$ detur. Theorematis auxilio erit $\frac{2dT}{4+T^2} \left(= \frac{dR}{RT}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{2dT}{4+T^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, ubi $\int \frac{2dT}{4+T^2}$ arcus est circuli, cujus sinus $\frac{T}{\sqrt{4+T^2}}$ et $-\int \frac{dp}{\sqrt{1-p^2}}$ arcus, cujus sinus $\sqrt{1-p^2}$, si arcus constantis C sinus sit c, erit $\frac{T\sqrt{1-c^2}+2C}{\sqrt{4+T^2}} = \sqrt{1-p^2}$, qua æquatione T in p invenire licet. Si $C = 0$, habetur in hoc casu speciali $T = \frac{2\sqrt{1-p^2}}{p}$ et per theorema 1. $dy = \frac{adx}{\sqrt{2ax+x^2}}$, curva igitur quæsita est catenaria.

Ex. 2. Quæritur curva, si $R = \frac{a\sqrt[4]{1+4T^2}}{2}$. Vi theorematis obtinetur $-\frac{2dT}{1+4T^2} \left(= \frac{dR}{RT}\right) = \frac{dq}{\sqrt{1-q^2}}$ et integrando $-\int \frac{2dT}{1+T^2} + C$

=

$= \int \frac{dq}{\sqrt{1-q^2}}$. Itaque quum arcuum $-\int \frac{2dT}{1+4T^2}$ et $\int \frac{dq}{\sqrt{1-q^2}}$ sinus sint $\frac{1}{\sqrt{1+4T^2}}$ et q respective, si arcus constantis C sinus sit c, prodit $\frac{\sqrt{1+C^2+2CT^2}}{\sqrt{1+4T^2}} = q$, qua T in q habetur. Si C = 0, erit $T = -\frac{\sqrt{1-q^2}}{2q}$ et per theorema 2. prodit $dx = -\frac{y^2 dy}{\sqrt{a^4-y^4}}$, unde constat, quod in hoc casu curva sit elastica.

THEOREMA V.

Manentibus adhibitis denominationibus et dicta DF, S, erit $\frac{ds}{ST} - \frac{dT}{T^2} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam 1 : T :: CD (R) : DF (S), erit $S = RT$ et $R = \frac{S}{T}$ ejusque fluxiones $dR = \frac{dS}{T} = \frac{SdT}{T^2}$. Quum vero $\frac{dR}{RT} = -\frac{dp}{\sqrt{1-p^2}}$, prodit substitutione $\frac{ds}{ST} - \frac{dT}{T^2} = -\frac{dp}{\sqrt{1-p^2}}$.

Schol. Mediante hoc theoremate indagantur curvæ, data relatione inter S et T. Si enim sit S = L, quantitatis T functioni, habetur $\frac{TdL-LdT}{LT^2} = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{TdL-LdT}{LT^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Ponatur $\int \frac{TdL-LdT}{LT^2} + C = m$ et N basis logarithmorum hyperbolicorum, erit $\sqrt{1-p^2} = \frac{N^{m\sqrt{-1}} - N^{-m\sqrt{-1}}}{2\sqrt{-1}}$, quæ functio est quantitatis T, quare T in p vel substitutione in q, r, &c. per hanc æquationem exprimi potest. Relatione adepta inter T et p vel q, &c. relatio inter coordinatas, vel inter curvam et abscissam vel ordinatam habetur, ut antea expositum est.

Generaliter

Generaliter conſtat, quod, quoties $\int \frac{T dL - L dT}{LT^2}$ non ſit per arcus circulares integrabilis, curva ſit tranſcendens.

Ex. 1. Si radius curvaturæ evolutæ $S = \frac{aT \cdot \overline{9+T^2}^{\frac{3}{2}}}{54}$, quæritur curva. Per theorema obtinetur $\frac{3dT}{9+T^2}$ $\left(= \frac{dS}{ST} = \frac{dT}{T^2} = -\frac{dp}{\sqrt{1-p^2}}\right.$ et integratione $\int \frac{3dT}{9+T^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Quum vero arcuum $\int \frac{3dt}{9+T^2}$ et $-\int \frac{dp}{\sqrt{1-p^2}}$ ſinus ſint $\frac{T}{\sqrt{9+T^2}}$ et $\sqrt{1-p^2}$, ſi arcus conſtantis C ſinus ſit c, erit $\frac{T\sqrt{1-c^2}+3C}{\sqrt{9+T^2}} = \sqrt{1-p^2}$ et reſoluta hac æquatione T in p habetur. Si ſit $c = 0$, erit $T = \frac{3\sqrt{1-p}}{p}$ et per theorema 1. $y = \sqrt{ax}$, curva igitur in hoc caſu eſt parabola Apolloniana.

Ex. 2. Quænam eſt curva, ſi evolutæ curvaturæ radius $s = \frac{aT \cdot \overline{9+4T^2}^{\frac{3}{2}}}{2\sqrt{27}}$? Theoremate habetur $\frac{6dT}{9+4T^2} = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{6dT}{9+4T^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Arcuum $\int \frac{6dT}{9+4T^2}$ et $-\int \frac{dp}{\sqrt{1-p^2}}$, ſinus ſunt $\frac{2T}{\sqrt{9+4T^2}}$ et $\sqrt{1-p^2}$, ſi arcus conſtantis C ſinus ponatur r, prodit $\frac{2T\sqrt{1-c^2}+3C}{\sqrt{9+4T^2}} = \sqrt{1-p^2}$, per quam T in p obtinetur, quæ, in caſu $c = 0$, dat $T = \frac{3\sqrt{1-p^2}}{2p}$ et theoremate 1. $dy = \frac{a^2 dx}{\sqrt{x^4 - a^4}}$ æquatio ad curvam, quæ conſtruitur rectificatione ellipſeos et hyperbolæ æquilateræ conjunctim.

THEOREMA VI.

Dicatur CF, U et reliquis manentibus, erit $\frac{dU}{UT} - \frac{dT}{1+T^2} = -\frac{dp}{\sqrt{1-p^2}}$.

Quum enim $-1 : \sqrt{1-T^2} :: CD\,(R) : CF\,(U)$, erit $R = \frac{U}{\sqrt{1+T^2}}$ ejusque fluxio $dR = \frac{dU}{\sqrt{1+T^2}} - \frac{UTdT}{\sqrt{1+T^2}^3}$, et quum $\frac{U}{RT} = \frac{dp}{\sqrt{1-p^2}}$, provenit substitutione $\frac{dU}{UT} - \frac{dT}{1+T^2} = -\frac{dp}{\sqrt{1-p^2}}$.

Schol. Auxilio hujus theorematis, curvæ inveniuntur, quando inter T et U relatio detur. Nam si sit $U = M$, functioni quantitatis T, erit per hoc theorema $\frac{1+T^2 \cdot dM - MTdT}{MT \cdot 1+T^2} = -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{1+T^2 \cdot dM - MTdT}{MT \cdot 1+T^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Itaque, posita basi logarithmica N et $\int \frac{1+T^2 \cdot dM - MTdT}{MT \cdot 1+T^2} + C = l$, erit $\frac{N^{l\sqrt{-1}} - N^{-l\sqrt{-1}}}{2\sqrt{-1}} = T$, quantitatis T functioni, quare inter T et p habetur relatio, per quam, methodo antea expositâ, relationem inter coordinatas vel curvam et abscissam sive ordinatam invenire licet.

Consequitur hinc, quod, quando $\int \frac{1+T^2 \cdot dM - MTdT}{MT \cdot 1+T^2}$ per quadraturam circuli non obtinetur, curva semper sit transcendens.

Ex. Si curva quæritur ubi linea CF sive $U = \frac{a}{2}$, ejus theorematis ope erit $-\frac{dT}{1+T^2} = \frac{dq}{\sqrt{1-q^2}}$ et integratione $-\int \frac{dT}{1+T^2} + C = \int \frac{dq}{\sqrt{1-q^2}}$.

Quum arcuum $-\int\frac{dT}{1+T^2}$ et $\int\frac{dq}{\sqrt{1-q^2}}$ sinus sint $\frac{1}{\sqrt{1+T^2}}$ et q si arcus constantis C sinus sit c, obtinetur æquatio $\frac{\sqrt{1-c^2}+CT}{\sqrt{1+T^2}}=q$, qua T in q datur, et si $c=0$, $T=\frac{\sqrt{1-q^2}}{q}$, quare in hoc casu speciali per theorema 2. habetur $dx=-\frac{2\sqrt{y}\,.\,dy}{\sqrt{a-2y}}$, æquatio pro cycloide ordinaria cujus circuli generatoris diameter $\frac{a}{4}$.

THEOREMA VII.

Si variatio curvaturæ evolutæ dicatur V ceteris manentibus, erit $\frac{dT}{V-T\,.\,T}=-\frac{dp}{\sqrt{1-p^2}}$.

Quoniam DM (1) : CD (R) :: $-\frac{dp}{\sqrt{1-p^2}}:dx$, habetur $dx=-\frac{R\,dp}{\sqrt{1-p^2}}$, quæ si multiplicetur per T prodit $d\mathrm{R}\,(=\mathrm{T}dx)=-\frac{\mathrm{RT}\,dp}{\sqrt{1-p^2}}$, et propter 1 : T :: CD (R) : DF erit evolutæ radius curvaturæ DF = RT, cujus fluxio $R\,dT+T\,dR$ per fluxionem evolutæ divisa dat ejus curvaturæ variationem $V\,(=\frac{R\,dT}{dR}+T)=-\frac{dT\sqrt{1-p^2}}{T\,dp}+T$ atque inde $\frac{dT}{V-T\,.\,T}=-\frac{dp}{\sqrt{1-p^2}}$.

Schol. Hoc mediante theoremate invenire valemus curvas, si inter curvaturæ variationes V et T relatio detur. Sit enim V = H, functioni quantitatis T, erit vi theorematis $\frac{dT}{H-T\,.\,T}=-\frac{dp}{\sqrt{1-p^2}}$ et integrando $\int\frac{dT}{H-T\,.\,T}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, si itaque ponatur $\int\frac{dT}{H-T\,.\,T}+C=l$ et N basis logarithmica, erit $\sqrt{1-p^2}=$

$\frac{N^{l\sqrt{-1}} - N^{-l\sqrt{-1}}}{2\sqrt{-1}}$, qua æquatione T in p vel ſubſtitutione in q, r, &c. exprimi poteſt, unde via, æquationem ad curvam inveniendi, patet.

Curva ſemper eſt tranſcendens, quoties $\frac{dT}{H-T.T}$ per circuli rectificationem non habetur.

Exempl. Sit evolutæ variatio curvaturæ $V = T + \sqrt{T^2-4}$, quæritur curva. Theoremate hoc habetur $\frac{dT}{T\sqrt{T^2-4}} \left(= \frac{dT}{H-T.T}\right) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{dT}{T\sqrt{T^2-4}} + C = \int \frac{dq}{\sqrt{1-q^2}}$ arcus, quorum ſinus ſunt $\frac{\sqrt{T+\sqrt{T^2-4}}}{\sqrt{2T}}$ c, et q, ſi arcus conſtantis C ſinus ponatur c, et exinde conſequitur $\frac{\sqrt{1-c^2}\sqrt{T+\sqrt{T^2-4}} + c\sqrt{T-\sqrt{T^2-4}}}{\sqrt{2T}} = q$, qua ſi $c = 0$ prodit $T = \frac{1}{q\sqrt{1-q^2}}$ et per theorema 2. $dx = \frac{dy\sqrt{a^2-y^2}}{y}$ in quo caſu curva eſt tractoria.

XXVI. *A Series of Observations on, and a Discovery of, the Period of the Variation of the Light of the bright Star in the Head of* Medusa, *called* Algol. *In a Letter from* John Goodricke, *Esq. to the Rev.* Anthony Shepherd, *D. D. F. R. S. and* Plumian *Professor at* Cambridge.

Read May 15, 1783.

SIR, York, May 12, 1783.

I TAKE the liberty to transmit to you the following account of a very singular variation in Algol or β Persei, which you will oblige me by presenting to the Royal Society, if you think it deserving that notice. All that has been hitherto known concerning the variation of this star, as far as I can find after the most diligent researches, is comprised in the following passage in DU HAMEL's *Historia Regiæ Scientiarum Academiæ, liber IV. sect.* 6. *caput VIII.* de rebus Astronomicis, ann. 1695, p. 362. "Id quoque testatur D. MONTANARI stellam lucidiorem "Medusæ, diversis annis, variæ esse magnitudinis: nullam pene "in eâ mutationem potuit advertere D. MARALDI annis 1692 "et 1693; sed anno 1694 aucta est et imminuta insigniter, modo "quarti, modo tertii, modo secundi ordinis stella apparuit." This, however curious, is only a very vague and general information; but the following observations, lately made, exhibit a regular and periodical variation in that star, of a nature hitherto, I believe, unnoticed.

The first time I saw it vary was on the 12th of November 1782, between eight and nine o'clock at night, when it appeared of about the fourth magnitude; but the next day it was of the second magnitude, which is its usual appearance. On the 28th of December following, I perceived it to vary again thus; at 5°½ h. in the evening, it was about the fourth magnitude, as on the 12th of November last; but at 8½ h. I was much surprized to find it so quickly increased as to appear of the second magnitude. My friend Mr. EDWARD PIGOTT, whom I informed of this singular phænomenon as soon as I saw it, also observed it; and I had the pleasure to find that his observations coincided with mine. The subsequent observations which I have made on this star are very particular; and I think it will be best to give a brief extract of them in their order from my journal; but it is necessary I should first specify the usual and greatest magnitude of Algol, as also the relative brightness and magnitude of those stars to which I compared it during the progress of its variation.

The usual and greatest magnitude of Algol is this; of the second magnitude, much less bright than α Persei, and not so much as γ Andromedæ; brighter than α Cassiopeæ and β Arietis, and nearly the same, if not rather brighter, than α Pegasi and β Cassiopeæ; not quite so bright as γ Cassiopeæ, and much brighter than ε Persei and β Trianguli. The relative brightness of the stars to which I compared it during the progress of its variation is as follows; α Cassiopeæ is the brightest, and of near the second magnitude; β Arietis is the next, and of between the second and third magnitude; then ε Persei and β Trianguli, both of the third magnitude; ζ Persei is somewhat less bright than ε Persei, and also of the third magnitude; δ Persei is less than ζ Persei, and rather of between the third

and

and fourth magnitude; ρ Perſei, which Algol is equal to at its leaſt brightneſs, is not ſo bright as δ Perſei, and of about the fourth magnitude.

OBSERVATIONS ON ALGOL.

Brightneſs and magnitude of Algol.

January 14, 1783.

At 6 h. it was varied from its uſual brightneſs, but rather brighter than β Arietis.

At 6¼ h. equal to β Arietis, but rather a little leſs bright, and of between the ſecond and third magnitude.

At 7¼ h. third magnitude; not ſo bright as β Arietis, and equal to β Trianguli.

At 7¾ h. nearly the ſame as at 7¼, but thought it was rather leſs bright than β Trianguli.

At 8¼ h. between the third and fourth magnitude; not quite ſo bright as β trianguli, and rather leſs than ε and ζ Perſei, but a little brighter than δ and ρ Perſei.

At 9¼ h. about the fourth magnitude, and equal to ρ Perſei.

The weather was cloudy till 11¾ h. when it appeared to be of the third magnitude; much brighter than ρ Perſei, and rather brighter than γ Perſei.

At 12¼ h. between the ſecond and third magnitude, and brighter than ζ and ε Perſei and β Trianguli.

January 17.

At 7¼ h. it was of the third magnitude, equal to ε Perſei, and rather leſs than β Trianguli.

At 8 h. a very little brighter than ε Perſei, and equal to β Trianguli.

At

At 8¼ h. rather brighter than β Trianguli, but the ſky was not favourable.

January 31.

At 10½ h. varied from its uſual brightneſs, but with ſome doubt.

At 11¼ h. certainly leſs bright; much leſs than γ Andromedæ, but brighter than ζ and ε Perſei, and of between the ſecond and third magnitude.

At 12¼ h. third magnitude, and rather brighter than ζ and ε Perſei.

At 13 h. about the brightneſs of ζ Perſei, and much brighter than ρ Perſei; but the ſky was not favourable.

At 14¼ h. about the fourth magnitude, and equal to ρ Perſei, but afterwards increaſed.

February 6.

At 5¼ h. it was rather a little brighter than β Arietis, and between the third and fourth magnitude.

At 6¼ h. about the third magnitude; not ſo bright as β Arietis, but brighter than β Trianguli and ε Perſei.

At 6½ h. about the ſame brightneſs as β Trianguli and ε Perſei.

At 7 h. between the third and fourth magnitude; not quite ſo bright as β Trianguli, nearly equal to δ Perſei, and a little brighter than ρ Perſei.

At 7¼ h. about equal to ρ Perſei, and nearly of the fourth magnitude; but the ſky was not favourable.

At 8 h. rather a little leſs bright than ρ Perſei; ſky ſtill unfavourable.

At 8½ h. between the third and fourth magnitude rather a little brighter than δ Perſei, and a little brighter than ρ Perſei.

At 9 h. certainly brighter than δ Perſei, and of the third magnitude.

At

At 9½ h. of the same brightness as ε Persei; but the sky was not favourable.

At 10 h. brighter than ε Persei.

At 10½ h. brighter than at 10 h. and of between the second and third magnitude.

At 11½ h. very bright; and now, as I think, at its usual magnitude.

On the 9th of February, at 6½ h. I thought it was less bright, and nearly equal to β Arietis; but have some doubts on account of the unfavourable sky.

February 23.

At 10½ h. it was brighter than at 9½ h. when I observed it at its usual brightness; now of the third magnitude, rather brighter than ε and ζ Persei.

At 11 h. about the same brightness as ε and ζ Persei.

At 12 h. between the third and fourth magnitude; not so bright as ε and ζ Persei, a little brighter than η Persei, and a little less than δ Persei.

February 26.

At 6½ h. between the second and third magnitude; rather less bright than α Cassiopeæ, but was not very certain.

At 9½ h. little less bright than ζ Persei, and of the fourth magnitude.

At 10 h. nearly between the third and fourth magnitude; a little brighter than η Persei, and a little less bright than δ Persei.

March 1.

At 8½ h. it was of the third magnitude; a little brighter than ε and ζ Persei.

At 8¾ h. brighter than at 8½ h.

At 9½ h. between the ſecond and third magnitude; a little leſs bright than α Caſſiopeæ.

At 10 h. I believe it now at its uſual brightneſs.

March 18, at 9½ h. Mr. E. PIGOTT thought it leſs bright; but the weather was very hazy.

March 21.

At 7½ h. it was about between the third and fourth magnitude; not ſo bright as δ Perſei, but brighter than ρ Perſei.

At 8 h. rather a little brighter than ρ Perſei, and ſometimes equal to it.

At 8½ h. about the fourth magnitude; equal to ρ Perſei; but ſometimes it appeared rather a very little brighter.

At 9 h. rather a little brighter than ρ Perſei.

At 10 h. about the third magnitude; equal to ζ and ε Perſei, but rather a little brighter.

At 10½ h. brighter than ζ and ε Perſei.

At 11 h. much brighter than ζ and ε Perſei; rather between the ſecond and third magnitude.

April 10.

At 8 h. it was about the third magnitude, and rather brighter than ε Perſei.

At 8½ h. nearly equal to ε Perſei, though rather a little brighter.

At 9 h. rather leſs bright than ε Perſei, but brighter than δ Perſei.

At 9½ h. rather leſs bright than δ Perſei, and between the third and fourth magnitude.

At 9½ h. about the fourth magnitude; not ſo bright as δ Perſei, but brighter than ρ Perſei.

At 10 h. rather leſs than at 9½ h.; believe it now very near its leaſt brightneſs.

April 13.

At 8 h. it was between the third and fourth magnitude; brighter than ρ Perſei, but not ſo bright as δ Perſei.

At 8½ h. rather brighter than δ Perſei, and not ſo bright as ε Perſei.

At 9 h. rather brighter than ε Perſei. It was too low to obſerve its farther variation.

May 3.

At 9½ h. nearly between the third and fourth magnitude, and ſomewhat brighter than ρ Perſei; but ſo low that I could not well compare it with other ſtars, or be able to obſerve the remainder of the variation. I believe it muſt have paſſed its leaſt brightneſs not long before.

The times of the above obſervations are nearly apparent time, and were for the moſt part made under favourable circumſtances. My friend Mr. EDWARD PIGOTT, to whom I am under great obligations on this as well as on other occaſions, alſo obſerved ſome of the variations; and where our times of obſervation were the ſame, always agrees with me.

From an attentive compariſon of all the particulars in the above obſervations it appears, firſt, that this ſtar changes from the ſecond to about the fourth magnitude in nearly three hours and a half, and from thence to the ſecond magnitude again in the ſame ſpace of time; ſo that the whole duration of this ſingular variation is only about *ſeven hours*. And, ſecondly, it appears alſo, that this variation probably recurs about every *two days and twenty-one hours*. This laſt concluſion will be rendered more conſpicuous by the following table; the firſt column of which ſhews the days, and exact time of the day, when Algol was obſerved to be very near, or at its leaſt brightneſs; the ſecond column marks the different intervals of time

elapſed between the ſeveral obſervations; the third exhibits the quotient ariſing from a diviſion of theſe intervals by a certain number of revolutions, each of two days and twenty-one hours, which number of revolutions are expreſſed in the laſt column.

The day and time when Algol was obſerved at or near its leaſt brightneſs.			The different intervals between the ſeveral obſervations.		The quotients of the diviſions of the 2d column by the 4th.		Number of revolutions.
	d.	h.	d.	h.	d.	h.	
1782 Nov.	12	8½					
			45	21	2	20,8	16
Dec.	28	5½					
			17	3½	2	20,6	6
1783 Jan.	14	9					
			17	5	2	20,8	6
	31	14½					
			5	17½	2	21,	2
Feb.	6	8					
			17	4	2	20,6	6
	23	12 +					
			2	21½	2	21,5	1
	26	9½					
			22	23	2	20,9	8
Mar.	21	8½					
			20	1½	2	20,8	7
April	10	10 +					
			2	22	2	22,*	1
April	13	8					
			20	1	2	20,7	7
May	3	9½					

The reſults in the third column agree ſo nearly, that there is the greateſt probability, not to ſay certainty, that the ſingular and quick variation of this ſtar, during the ſpace of ſeven hours, as above mentioned, recurs regularly and periodically about every two days and nearly twenty hours and three quarters.

To aſcertain this period with greater accuracy and preciſion will require more time and obſervation: but I can add, that I

* The difference of upwards of an hour in this quotient will eaſily be reduced to the others by remarking, that Algol was obſerved on the 10th and 13th of April, not when it was *at*, but only *near*, its leaſt brightneſs: and, indeed, all the little differences of the reſt will vaniſh by making a reaſonable allowance of the ſame kind.

have conſtantly obſerved Algol, at different times, every night when the weather permitted, ever ſince the 28th of December laſt; and upon accurately examining all theſe obſervations in my journal, I find, that ſo far from containing any appearances the leaſt contrary to the above concluſion, they ſtrongly corroborate it, ſince I never obſerved that ſtar varied in any of thoſe days which, according to that theory, were the intervals between its variations. All Mr. EDWARD PIGOTT's obſervations, even at different times from mine, tend to confirm the ſame concluſion.

Whether this ſingular phænomenon is always the ſame; or whether it occurs only ſome years, and ceaſes intirely in others (as may be preſumed from the account of MONTANARI and MARALDI above quoted); and whether in this caſe it recurs in regular periods of time or otherwiſe; are curious objects of inveſtigation, which can only be determined by a long and regular courſe of obſervations for many years.

If it were not perhaps too early to hazard even a conjecture on the cauſe of this variation, I ſhould imagine it could hardly be accounted for otherwiſe than either by the interpoſition of a large body revolving round Algol, or ſome kind of motion of its own, whereby part of its body, covered with ſpots or ſuch like matter, is periodically turned towards the earth. But the intention of this paper is to communicate facts, not conjectures; and I flatter myſelf that the former are remarkable enough to deſerve the attention and farther inveſtigation of aſtronomers.

I am, &c.

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ERRATA

IN

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Page.	Line.	*for*	*read*
120	1	near FL. 19.	FL. 20.
	30	60° 55′	61° 23′
127	8	FL. 198. In seq. flexu 5ª	FL. 107. In seq. flexu 4ª
	17	Posit. 28° 17′	Posit. 31° 3′
132	15	FL. 1.	near FL. 1.
133	24	1′ 7″ 49‴	57″ 49‴
136	24	ξ Capricorni, FL.	FL. 64. Sagittarii
139	8	2 ad ω Cygni, FL. 45.	3ª ad ω Cygni, FL. 46.
	12	3 ad ω Cygni, FL. 46.	5ª ad ω Cygni (FL. 46ª) adjacens
	18	In Constell. Ceti,	In Constell. Ceti, FL. 66.
140	5	FL. 44.	FL. 43ª præcedens ad boream
144	21	Posit. 80° 47′	Posit. 79° 51′
145	3	Posit. 40° 58′	Posit. 5° 36′
146	22	near FL. 28.	A, FL. 28.
149	22	θ northwards	θ southwards
150	18	FL. 35.	FL. 41.
	26	FL. 59.	FL. 56. and 57.
151	16	73° 39′	75° 21′
153	11	Posit. 33° 42′	Posit. 61° 23′
154	7	2′ 6″ 12‴	2′ 41″ 46‴
	27	1′ 5″ 10‴	59″ 4‴
156	4	FL. 8.	FL. 4.
161	21	6′ 27′	6″ 27‴

ER-

ERRATA.

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Page. Line.

303. 1. *for* poſſible *read* fuſible

405. 6. *for* theſe *read* thoſe

406. 5. *after* phlogiſticated *add* air

407. 28. *before* cobalt *add* of

408. 1. penult. *for* neceſſarily *read* neceſſary

412. 6. *for* equally *read* equably

413. 8. *after* dephlogiſticated *add* air

418. 8. *after* imagined *add* that

421. 13. *for* come *read* came

423. 8. *dele* a *before* water

475. 7. *for* $5^{o}\frac{1}{2}$ h. *read* $5\frac{1}{2}$h.

— 24. *for* not quite ſo bright as γ Caſſiopeæ *read* rather leſs bright than γ Caſſiopeæ.

476. 18. *for* between the third and fourth magnitude *read* between the ſecond and third magnitude.